SCIENTIFIC AMERICAN SUPPLEMENT No. 283

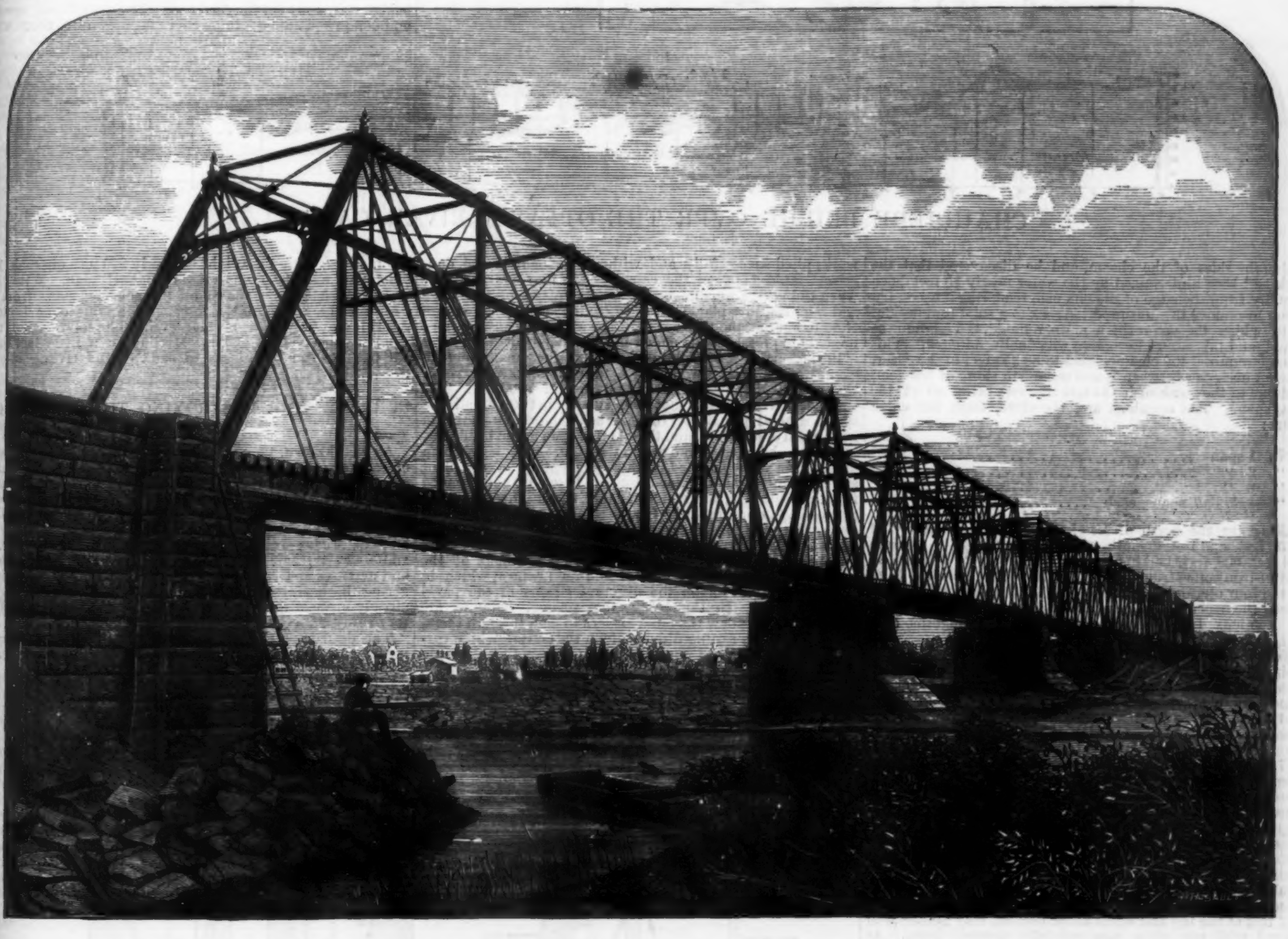
Scientific American Supplement. Vol. XI. No. 283.
Scientific American, established 1845.

NEW YORK, JUNE 4, 1881.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

CHAUDIERE BRIDGE, OTTAWA.

THE difficulties to be overcome in building a bridge in the neighborhood of Ottawa city were unusual, and where expense was a necessary consideration, almost appalling. The Ottawa is not only one of the largest rivers on the continent, seldom, in any part of its course, less than a mile in breadth, but it is subject to sudden and dangerous rises of water, to wild floods, and at the season when the ice breaks up, to terrific jams and destructive conditions, which render the permanence of any work, however strongly constructed, in most situations hazardous and uncertain. From Aylmer, eight miles up the river from Ottawa, is a continuous dangerous rapid, which terminates at the capital in the celebrated Chaudière Falls, and immediately below these the river expands into a broad deep basin, the crossing of which for miles below the city would be attended with enormous expense, and involve a very high, as well as a very long bridge. The question was therefore between a shorter bridge at a lower level in the dangerous rapids above the falls, or a long high structure in the deeper but still water below them, where the river widens out to its normal breadth; and after carefully weighing all the considerations the upper site was chosen, and the work has been satisfactorily completed, and with a very much less loss of life or property than usually takes place in the construction of a work of this magnitude, apart from the unusual dangers that made this situation apparently so extremely hazardous. The route as finally selected by Mr. Peterson, the engineer, leaves the Quebec and Occidental Railway at the Hull Station, 117 miles from Montreal, and 264 miles from Quebec, and is a little over $1\frac{1}{2}$ miles in length, to a junction with the Canada Central, just north of Ottawa city, and about half a mile from their present station at the capital. The river at this point is barely three fourths of a mile in width from shore to shore, and near the center of the river is the rocky Lemieux Island, which is usually some few feet above the water, but completely submerged in the spring and early summer, when the foaming freshet dashes wildly over it. The site selected is only 350 yards distant from the great cataract of the Chaudière, but it has the advantage of crossing the river where the current is less wild and unmanageable than in almost any other portion of the broken water from Aylmer to the falls. In ordinary conditions the current under the bridge is only two miles per hour, not really dangerous to a well-handled boat or barge, and not materially increasing the expense of the cofferdams and false works. Were it not for the great roaring cataract immediately below there was nothing in the current or in the nature of the river that had not been encountered in the longer and loftier bridge over the St. Lawrence at Montreal, but in the immediate presence of the remorseless fate that awaited the slightest indiscretion or mischance, this Chaudière Bridge is one of the boldest conceptions that has yet been carried out on any railway in existence. The main channel of the river is near to the north shore about 200 feet in width, and varying in depth from 44 feet to 56 feet at different periods of the year. The remainder of the river varies in depth in ordinary summer water from 3 feet to 10 feet, to which at least 7 feet must be added during the months of May and June.



THE CHAUDIERE BRIDGE, OTTAWA RIVER, CANADA.

The bed of the river is composed of limestone rock, and presents a very broken and irregular surface, in some places level, smooth, and water-worn, in others broken off in irregular steps with transverse and vertical fissures, and intrusions of a harder and more intractable material, whilst the immediate sides of the main channel have a jagged and serrated edge, as if the softer portion of the rock had been worn away by the action of the water, leaving the unsupported projecting masses to be broken off by their own weight or by pressure from above. These projecting lodges of rock, with a free course for the water underneath, together with the numerous vertical cracks and fissures, appeared to render the usual process of securing a foundation for the piers by cofferdams and pumping difficult if not impracticable. In practice, however, the vertical fissures were generally avoided, and most of the foundations were unwatered without the difficulties being very serious. The time, however, during which operations could be carried on under water was limited. The water of the Ottawa commences to rise as soon as the ice breaks up in April, and increases in volume until it reaches its maximum in the middle of June, during the whole of which period the rush of water over the proposed bridge site is tremendous, and sometimes terrific. For a month after this, as successive great rivers in the vast unexplored north break up from their winter fetters, the "freshet" continues, and work on the verge of the Chaudiere cataract would be impossible, and it is generally after the middle of July before it assumes its normal condition and that works can be prosecuted. From then to the middle of December gives but a short season of five months in which these heavy foundations could be undertaken.

With all these difficulties and vast forces to contend with, and on the immediate verge of a cataract, not inferior in volume of water in the spring of the year, and scarcely even in sublimity and beauty, to Niagara itself, the rapidity with which this massive "Prince of Wales Bridge," as it is now proposed to call it, has been erected, is not one of the least interesting of the many engineering features that mark the whole grand design. The first surveys were commenced in February, 1879, and before the end of the next year the locomotives were working across it. Owing to the severity of the previous winter, the ice had formed over the river from each shore, leaving only 300 feet of the channel open. The frozen surface was marked out in squares of 50 feet extending on to the shores on either side, and accurate soundings were taken from this grand datum level for a distance of 800 feet above and 300 feet below the bridge site, and in places presenting any difficulty these preliminary trials were made every 25 feet, all being carefully plotted and connected with the fixed marks on the river banks. With this information the center line was carefully marked out on the ice, the position of the various piers determined and marked off, and fresh soundings made at every point of the intended work. The highest known elevation of the water in the freshet season was ascertained to be 55·65 feet above the rock bottom of the channel, and the lowest portion of any part of the superstructure was decided to be 9 feet above this, or 64·65 feet above the bed of the river. The piers are finished off 9 inches below this for bedplates and expansion rollers, which gives a uniform height of the top of the piers and bridge abutments 63·90 feet above datum, and allowing 2 feet 9 inches for the depth of back stringers, 9 inches for the flooring beams, 4 inches for the rail, $67\frac{3}{4}$ feet is the actual height of the top of the rail from the bed of the river. The piers and abutments are all at right angles to the axis

of the bridge, and the piers all finished off at the top 8 feet wide by 24 feet long, while the bridge seat on each of the abutments is the same length and 4 feet wide.

The position of Lemieux Island, and the fact that it practically diminishes the waterway of the river, determined the engineer to build a permanent embankment across this island, with an abutment at each side, thus saving about 1,000 feet of bridge work, and dividing the structure into two nearly equal parts. The bridge was designed to be of the form known as a pin-connection through-bridge; that is, the trains were to run between the trusses on the bottom chord, with a clear headway of 20 feet from the top of the rails to the lowest part of the overhead bracing, giving ample headroom for a man standing on the top of an ordinary box car, while the clear distance between the trusses for the passage of a train was fixed to be 15 feet. As originally intended, the spans would have been more uniform in length, but before commencing No. 2 pier, which stands near the edge of the channel, a diver was sent down to examine the sides of the chasm, and he discovered that the rock had been undermined by the water nearly 15 feet horizontally from the edge of the channel, rendering it necessary either to remove this unsupported rock to a solid foundation, or to widen the span till a more reliable bed should be found, and this latter alternative was finally adopted. This change required the channel span to be 255 feet in the clear, leaving one span between it and the north shore of 160 feet, and one to the south of it 135 feet, the rest being all of 150 feet span. The openings are, therefore, from the north shore:

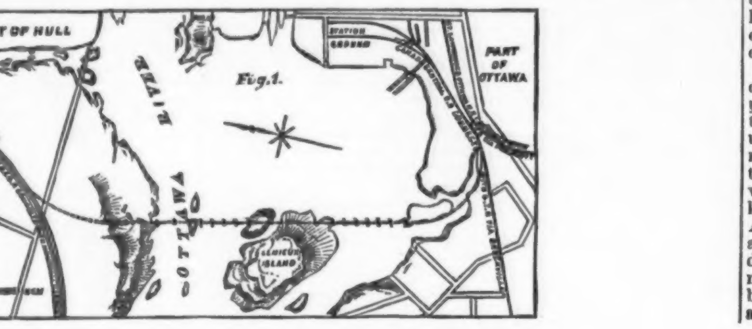
	Feet.
No. 1 span	160
" 2 "	255
" 3 "	135
" 4, 5, 6, 7 spans, each	150

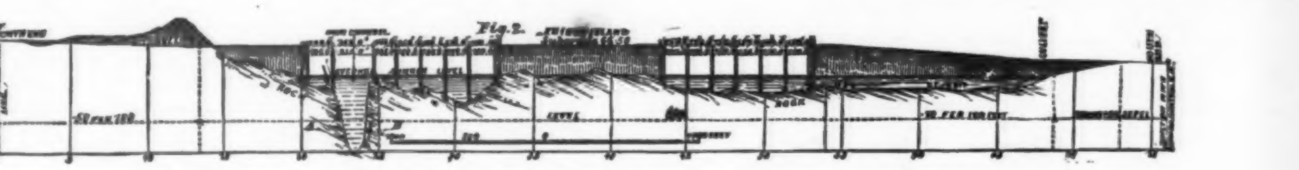
making, to the north of Lemieux Island, a waterway of 1,150 feet, and to the south of it, spans Nos. 8 to 13, each 150 feet, 900 feet, making a total waterway of 2,050 feet, and of iron superstructure of 2,154 feet. From back to back of the four abutments is a total length of 2,280 feet, and between the backs of the shore abutments, including the work on Lemieux Island, 3,400 feet, or over five-eighths of a mile.

In the spring of 1879 a contract was let to Mr. H. J. Beimer, based on a schedule of prices, but amounting altogether to $112,875 for the whole of the work ready for the superstructure; work commenced in the following May, and as soon as the flood subsided on the 17th of July the masonry was begun. During the summer work was in progress on all four abutments, and on piers Nos. 1, 2, 3, 4, 7, 9, 10, and 11, and 3,754 cubic yards of masonry were permanently placed in the structure. The foundations of these were all got in by the usual device of cofferdams and pumping, and as the depth of water did not exceed for these piers 7 feet, this method was tolerably successful, although Nos. 1 and 4 required great labor and excessive pumping before they could be sufficiently unwatered to get the masonry in place. These cofferdams were formed of 6-inch timber, braced across every 7 feet, and having an outside sheet piling of plank, between which and the timber, about 2½ feet at the top and 4 feet at the bottom, clay puddle well mixed with pea straw was carefully rammed down. At piers 5, 6, and 8, however, this system utterly failed, and though the cofferdams were put in on a much heavier design than the above in shallower depths they were never cleared of water and next spring disappeared over the falls. At these piers the water varied from 12 feet to 16 feet in depth, and the bottom was rugged and uneven. The method of forming a water-tight space to commence the masonry was to frame a bottomless caisson the sides and ends of which were "scribed" to fit the inequalities of the rocks at the bottom, and being rendered elsewhere water-tight were lowered into their places and weighted with stone. Into these caissons concrete was lowered by means of a wooden box which held half a yard. About midway of the depth of this box, a hinged bottom was fitted which, opening in two halves, deposited on the rock bottom within the caisson the contents of the upper portion of the box, in the still water within the lower half, allowing the concrete to be deposited without any sensible washing away of the cement, while the buoyancy of the box when relieved from its load quickly brought it up to the surface. By this means a layer of concrete was laid in the bottom of the caisson from 3 feet to 6 feet in depth, and this formed an artificial water-tight bottom for the caisson, in which after being pumped dry the masonry was laid without difficulty. Some experiments were made to determine the best proportions for the concrete, and also the time that was necessary to set it sufficiently to resist the disturbance of the pumps and the upward pressure of the water when emptied. As a result of these tests, the concrete was of broken stone mixed with one to one of sand and Portland cement, and after three days it was sufficiently dry to permit the caisson to be pumped out, while the surface was so hard that it required to be "pointed off" before commencing the masonry. The bottoms of Nos. 5 and 6, in 14 feet and 16 feet of water, were thus secured during the month of August, while the same operation at No. 8, commenced in March previously, had been excessively tedious. As the coldness of the water was apparently the cause of this, it was suggested to use steam to heat the concrete before submerging it; this proved to be efficacious in assisting the hardening, and with the use of steam a week sufficed in the coldest water to set the concrete. The caissons were so arranged that after the masonry was complete, by releasing some bolts inside, the upper part floated off, and after this was removed the foundation was protected by hand-laid rip-rap. The stone for the piers was quarried not far from the works, and was taken to each pier by scows towed by a steamer. This vessel, whose limits of navigation were probably more circumscribed than any vessel upon record, as the distance between the Little and Grand Chaudiere is scarcely half a mile, was built upon the small slope of water upon which she was to work, plied successfully and without accident in the dangerous rapid between the cataracts for two seasons, and when the bridge was finished, was drawn out of the river and removed to a placid lake, where, apparently, as secure from danger as she had formerly been in its constant and immediate presence, she ended her brief existence by fire.

In the spring of 1880 the contract for the superstructure was awarded to the well-known firm of Clarke, Reeves & Co., of Philadelphia, for the lump sum of $193,078. The specification required that the superstructure of all the spans, excepting that over the channel, should be of uniform height and description, and the 255 feet span might be higher, but was to be of the same general design. The usual plan of the firm was adopted, with the exception of the floor system, which was designed by Mr. Peterson, and applied first in this bridge, but which Messrs. Clarke, Reeves & Co. propose to adopt in their future structures. In the usual design the cross girders under each post are suspended by loops hanging from the same steel pin which receives the combined strain of the different tension members of the panel, and supports the Phœnix column or post, which in turn carries from its top the different strains of the panel on either side. In this bridge the vertical columns are slotted so as to allow the cross girders to pass through them, the girders thus resting directly upon the casting which carries the column, and through which passes the steel pin receiving the different chords and braces. The arrangement saves in this bridge 22 inches in the height of the piers and abutments, giving the same head-room above the water, and effecting a stiffer and more efficient arrangement of the floor. Riveted to the cross girders by angle irons are four backstringers, 4 feet centers and 2 feet 9 inches deep, which run the full length of each span, and resting upon these are a series of wooden sleepers or needle beams, 14 feet long, 8 inches by 9 inches in section, and spaced 8 inches apart, to which the rails are spiked on each side. Outside of the rails, and 7 feet 6 inches apart, are two longitudinals, 6 inches by 7 inches, let into the needle beams 2 inches, and flush with the top of the rails, and on the inside of these an angle iron 3 inches by 3 inches, weighing 22 lb. per yard, is fitted as a guard rail in case of the train leaving the rails. Outside of this again, 11 feet apart, are two longitudinal balks of timber, 12 inches by 12 inches, let into the needle beams 3 inches as an outer guard rail and walking track, and bolted down to every second sleeper, the whole being a safe, light, and fireproof arrangement, strong enough to carry even an engine in case of derailment, and yet free from any accumulation of snow or ice in the winter, or danger of communicating fire from one portion to the other in case of burning embers falling from a passing engine.

The superstructure was erected by means of false works of timber built from the bed of the river forming a temporary bridge, but some difficulty was experienced in getting this into place for the channel span, the mean current under which, at the time of its erection, was at least four miles per hour. To counteract this the trestles were framed together on the shore, the lower portion heavily weighted with rails, and then lowered, while the upper portion was kept in place by guy ropes. The first span was swung August 28, 1880, and afterward four days sufficed on an average to get each of the others into position. Some delays, however, subsequently took place in the delivery of materials, and it was the 14th of December before the bridge was ready for the final tests. These were very severe, and comprised among others covering the entire floor with locomotives, giving a weight of nearly 200 tons on each 150 foot span, and 300 tons on the channel span. With this weight the deflection varied on the smaller spans from ¼ inch to ⅜ inch, and on the long span a trifle over 1 inch, returning exactly to its normal level. The total cost of the whole has been about £62,000 sterling, or a little less than £18 per foot run from bank to bank of the river.—*Engineering.*



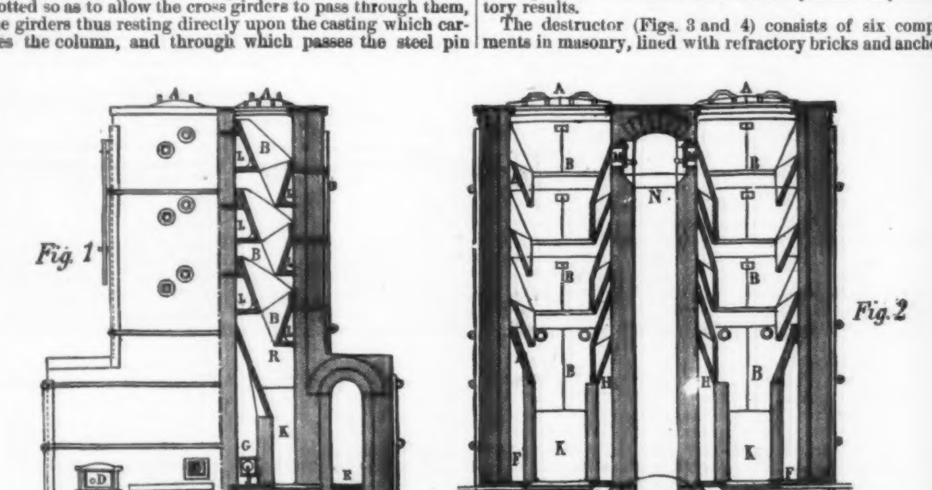


CHAUDIERE BRIDGE OVER THE OTTAWA RIVER, CANADA.

FURNACES FOR DESTROYING THE REFUSE OF CITIES.

As well known, there are numerous difficulties in the way of satisfactorily disposing of the refuse of all kinds which daily accumulates in cities, and the transportation of such material to a distance, where it may be dumped without danger to the public health, is attended with considerable expense. Experiments have been made at Burmantofts with an apparatus represented in the accompany Figures 1, 2, 3, and 4. It is the invention of Mr. Fryer, of Nottingham, and destroys and carbonizes refuse matters without endangering the health of the neighborhood. The experiments already made with it are said to have yielded very satisfactory results.

The destructor (Figs. 3 and 4) consists of six compartments in masonry, lined with refractory bricks and anchored



FURNACES FOR DESTROYING THE REFUSE OF CITIES.

by iron rods. It occupies a space 2 feet by 24 and is 12 feet in height. An inclined plane leads up to a platform situated at the top of the apparatus, and up which the refuse is carried. Another inclined plane, beginning on a level with the platform of the fire-place, is used for carrying off mortar, coal, old iron, etc. Each of these compartments permits of the destruction, every twenty-four hours, of 7 tons of refuse, and is composed of an inclined furnace with a dead-plate, C, and a grate, D, covered over by a dome, E, of refractory bricks. An aperture, A, serves for the introduction of the refuse, and another, G, allows the gases to enter the flue. The ashes and scoriæ are removed through the doors, F. The refuse, which is thrown by shovel into the compartment, falls on an inclined plane, B, situated above the dead-plate; then it slides over the latter, and when it is sufficiently dry, falls on the grate, where it is burned. The aperture, H, is separated from the gas-outlet by a wall, a fire-bridge, K, preventing also the refuse from passing into the flue. The ashes are removed from the furnace at intervals of about two hours each, and a new charge thrown in. The materials are thoroughly burned up and transformed into scoriæ or fine ashes. In one of the two adjacent compartments there is an aperture, P, for the introduction of infected bedding, putrid or diseased meat, etc., directly into the fire-place, where these matters are consumed without giving off any odor. The gases on their way to the chimney pass around a multitubular boiler 6 feet in diameter by 10 feet long, wherein the steam is generated for driving a horizontal engine having a cylinder 10 inches in diameter and a stroke of 20 inches, and which actuates two mortar mills. The ashes formed in the destructor are mixed in these mills with lime, and ground up to form a very strong mortar, which finds a ready sale at about $1.25 per cart load. No combustible has to be used, the ashes mixed with the refuse being amply sufficient. The iron and tin which have passed through the furnaces are sold as old metal. During the year there were burned in the destructor at Burmantofts 14,000 tons of refuse, 59 beddings, 131 mattresses, 264 diseased hogs, 1 cow, 8 sheep, 2 lambs, and 28 quarters of meat. The total quantity of refuse material burned in two and a half years was 30,041 tons. For each plant there are required one superintendent, who is at the same time machinist, and four workmen for the furnaces, and one who also takes charge of the mortar mills. A second relay of the same number of men do night work.

The carbonizing apparatus is employed for transforming street and store sweepings and other vegetable refuse into a charcoal which is very useful as a fertilizer and as a disinfectant, and which sells at $7.50 per ton. The apparatus (Figs. 1 and 2) is composed of a group of compartments and furnaces in masonry, in which each compartment is provided with a special furnace located at the side. Its length is 25 feet, its width 12 feet, and its height 15 feet. It is anchored by iron rods and angle irons. The refuse to be carbonized is thrown in at the top of the apparatus, through an aperture whose cover, A, is at once closed after the apparatus is charged. In the interior of the masonry compartments there are suspended by means of cast iron wall plates a series of cast iron inclined planes, so arranged as to cover one another and form a continuous spiral from the top to the bottom of the compartment. At the base of the latter this spiral terminates in an inclined refractory piece, R, which rests against the wall that separates the contents of the compartment from the hot gases of the fire place which enter from the other side.

The refuse matters are thrown into the compartment until they form a solid mass in the well formed by the spiral channel. This mass is drawn to the base when it has been sufficiently calcined, but it has not sufficient mobility to rise under the inclined planes or behind them; so there remains a free space there which forms a continuous flue, L, which is connected with the chamber, G, behind the refractory piece at the base of the compartment. The hot gases coming from the fire place ascend this flue and heat the contents of the compartment. The gases having reached the upper portion of the latter pass through the register, M, into the vertical flue, N, and from thence into the principal flue, P, which leads them to the chimney. After being thrown in at the top of the compartment, the refuse matters descend gradually and come in contact with hotter and hotter plates, and finally enter at the base of the compartment, a chamber lined with fire bricks heated almost to redness. No air has access to the interior of the apparatus during the operation, save the small quantity which enters through the flue behind the inclined planes; so the material is calcined instead of being burned. The compartment terminates about 2 feet above ground in a strong cast iron plate containing an aperture, which is closed by a sliding door, O. At certain intervals, say of about three hours, this door is opened to allow of the escape of some of the charcoal, which drops into a cart standing beneath the plate. The furnace, with its grate, E, and its door, D, is constructed in the usual way. In the flues, F, G, and H, situated near the fire place, there are apertures, J, through which the flues may be cleaned, and other similar ones allow the workmen to watch the inclined planes of the upper part of the compartment and see that they do not get too hot. Although the plates, B, are bolted to the walls of the compartment or connected with each other through the walls, they may be removed when necessary without touching the masonry. The charcoal which comes red hot from the apparatus is cooled in a special apparatus consisting of a revolving cylinder upon which cold water is kept continuously flowing. This apparatus, like the mortar mills, is actuated by a steam engine. Each compartment receives every twenty-four hours about 5,500 pounds of refuse matter, which furnishes all the fuel necessary for the furnaces. The cost of erecting one plant, consisting of one destructor of six compartments, one carbonizing apparatus, one steam engine, two mortar mills, one cooler, and the building, is about $22,500. The apparatus has been adopted at Kralingen, near Rotterdam, at Leeds, at Heckmondwicke, Blackburn, Bradford, Warrington, and Derby.

ON THE STABILITY OF CERTAIN MERCHANT SHIPS.

THIS paper, read before the recent session of the Institution of Naval Architects, by Mr. W. H. White, gave the results of a series of calculations made at the Naval College from data furnished by drawings of several typical merchant ships, and by inclining experiments made therewith by the builders for the purpose of ascertaining the vertical positions of the centers of gravity of those ships in a completed state. From data thus obtained estimates were made of the vertical position of the centers of gravity corresponding to various conditions of loading. From the information thus furnished a number of metacentric and stability curves were constructed. Several of these curves were given in the paper, and the author explained their use in affording comparisons between naval and merchant ships, and showing the relative changes of position of the metacenter and center of buoyancy with change of center of gravity consequent on loss or movement of cargo or consumption of coal. He explained that with many long, narrow, and deep merchant ships the greatest care had to be taken when unloading a cargo, coal being taken in as the vessel becomes light. Reference was made to the use of water ballast in order to make up for the stiffness lost by removal of cargo and coal. It was pointed out, however, that "there seems good reason for thinking that extreme narrowness in proportion to length and draught does not promote economy of steam power; and the movement in favor of greater proportionate beam which is now taking place will have the effect of increasing the initial stability of merchant steamers in the light condition." The experiments of the late Mr. Froude were quoted as showing that with a constant length, draught, and displacement, the extreme breadth of a ship may be increased from a little over one-tenth the length to a little over one-eighth of the length, without increasing the resistance at moderate speeds, and with a decrease in resistance at high speeds. The fining of the ends of the vessel, and the rounding out of the water lines at midships which always accompanies this change of form of the ship, gives greater moment of inertia to the load line section, or plane of floatation of the same area; the height of the metacenter above the center of buoyancy and above the keel is at the same time increased. The very great difference between the conditions of stability in different types of merchant ships shows the necessity for determining those conditions for each type, and this is recognized by some builders who make a practice of experimentally determining the initial stability of the ships they build.

Mr. W. Denny opened the discussion on Mr. White's paper, and said that some firms set off the metacentric height in terms of the moulded breadth, and adopted a minimum metacentric height of 0·4 of that breadth, making the calculation on the assumption that the coal was consumed. He said that the great increasing attention now being paid to stability of merchant ships was a matter for congratulation and encouragement. Mr. Martell spoke of the great importance of the question of stability, and remarked that some of the very long narrow ships weighed 0·6 of their load displacement, and, indeed, this was shown by reference to a table in the paper which gave the light displacement of one of the vessels as 1,760 tons, and the load displacement as 2,870 tons, or 61 per cent. of light load. Froude, he said, had showed that greater width for ships might be advantageously employed, and another speaker said that greater width should be in every way encouraged, for ship after ship is lost for want of stability only. This speaker also remarked that the construction of the lighter structures on the deck should be encouraged, so as to get the center of gravity low. Mr Barnes remarked that the relation of the breadth of a ship to the length might have little to do with the stability unless the depth were taken into account, the depth being the more important factor.

ON WAVES RAISED BY PADDLE STEAMERS AND THEIR POSITIONS RELATIVELY TO THE WHEELS.

The object of the author, Mr. Hamilton, was to consider two sources of loss of power in paddle-steamers; these were, energy absorbed in creating and maintaining waves, and the effect of this wave formation on the wheels. The first of these, due to constant wave making, is common to all vessels pushed to a speed beyond that for which their size and form fit them. It is irrespective of the description of the propeller, and, according to the late Mr. Froude, forms a large proportion of the total resistance. The author then explained that a ship requiring much propelling power sets up a wave formation which upsets the calculations upon which, with an assumed water line, the diameter and position of the wheels were theoretically fixed.

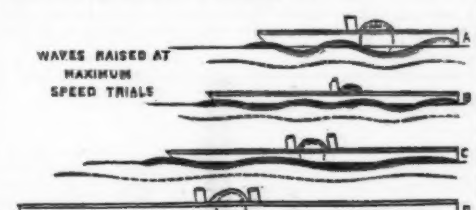


FIG. 1.

Fig. 1 was given as illustrating these causes of loss of power. It represents four vessels, marked A, B, C, and D respectively, and the waves that each sets up in smooth water at her maximum speed. The speeds are—A, 11·4 knots; B, 10·88 knots; C, 16·18 knots; and D, 17·9 knots, or 20·61 miles. Exhaustive trials on the same measured mile were made with each of the vessels. The types are widely different, and notwithstanding any imperfections that may be shown to belong to any of them, each vessel is among the fastest, or is the fastest, of her type and class. The first, marked A, raises three waves in the length, and brings the wheels into a wave hollow, thereby reducing the area of the paddle race considerably, and the center wave curves too close behind the wheel to make the increased head of water of use in giving forward pressure to the vessel. The second, marked B, is a case of four waves in the length, a wave coming just in front of the wheels, the one abaft it being in this case also too far forward to be of service. The third and fourth, C and D, have about two and a-half waves in the length, the wheels working at the normal level. These vessels are of a length and form more nearly corresponding to their speeds, and so the waves once raised will travel along with the ship without a great expenditure of power to maintain their speed. In the case of the first two, however, as the dotted wave lines drawn under each vessel show, the length of rolling waves corresponding to the speed is in each case much greater than the waves made by the ship, so that short waves are made to travel at a rate beyond their natural speed, and a constant expenditure of power goes on to accomplish this.

The vessel marked A is the shortest and fullest of the series, and was driven at the greatest speed in proportion to her length and form. This vessel was built in 1875 for heavy towing work abroad, and was tried on the measured mile. The speed was only about 10½ knots. The following day a second trial was made with the ship more by the head, but with no better results. The speed was not satisfactory, and, as the boiler could not apparently give sufficient steam, it was about to be condemned, and one with more fire-grate substituted. It was not, however, clear that a new boiler would improve matters, so it was determined to subject the ship to careful trials at different speeds, and to note the wave formation, with the object of discovering whether the speed could be improved by increasing the boiler power.

When the speed and power curves shown in dotted lines

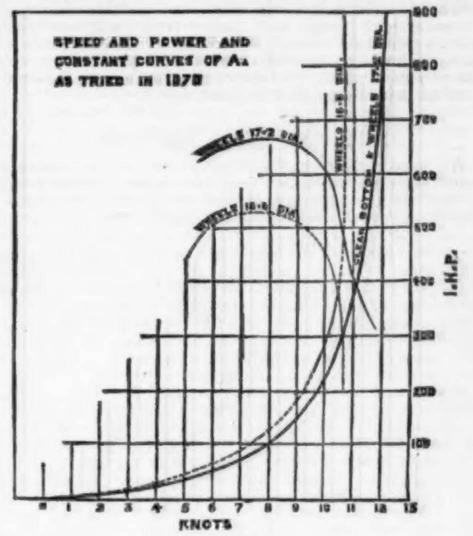


FIG. 2.

on Fig. 2 were drawn, it was seen that no multiplication of power with the same wheel could increase the speed.

Curves of revolutions and slip being made—Fig. 3—pointed to a sudden falling off in the efficiency of the propellers at the speed corresponding to the elbows in the curves, and when the wave crests and hollows, as shown in

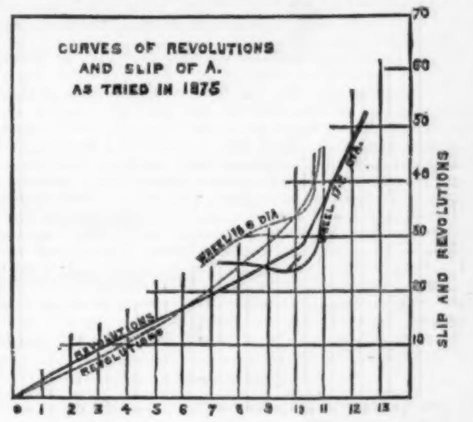


FIG. 3.

Fig. 1, were set off, the cause was explained. The water, which was at its normal level about 10 knots, fell off about 15 in. at 11·4 knots, thus reducing the area of the paddle race by nearly one-half. The remedy seemed to lie in increasing the diameter of the wheels. This was done temporarily by bolting iron plates on the outside edge of each float, increasing the diameter 18 in. This was done and another trial

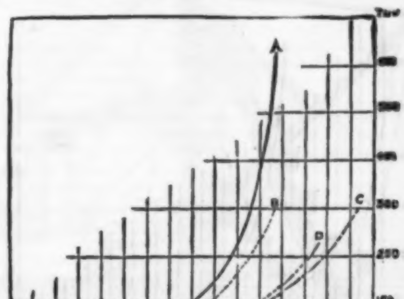


FIG. 4.

made. The other conditions were the same on this as on the previous trial, except that the vessel was put on the slip and the bottom cleaned and painted with a mixture of half paint and half tallow. The gain in speed was three-quarters of a knot, a half knot being due to the cleaned bottom, leaving one-quarter of a knot as the result of increasing the diameter of the wheels. The curves belonging to this trial are

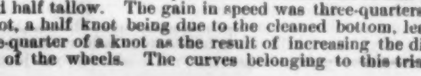


FIG. 5.

shown on Figs. 2 and 3 by lines in full. Fig. 1 shows that broad, full vessels exert power in raising a series of waves, whereas longer vessels of narrower and sharper design are more or less free from this.

The curves in Fig. 4 give the comparative force required in each case in propelling the respective types at a given speed assuming all the vessels to be of the same length. At 11 knots A takes four times the power of C and D, and twice that of B.

The author considers that, from the data derived from the trials, his paper shows the difficulty of driving full types as compared with finer ones.

Fig. 5 is intended to show how little wetted surface affects the resistance in vessels of this class, and that surface is a most unreliable unit of comparison.

While in the preceding paper reference was made to the advantage of a return to broader vessels, Mr. Hamilton concludes by saying that his paper shows "the great waste of power in wave making in short, full paddle-steamers, as compared with longer and sharper types; and that much may be gained in economy by increasing the proportion of length to breadth and depth in vessels of this class, the success of vessels of this kind depending far more on their proportions than any fanciful form of lines."

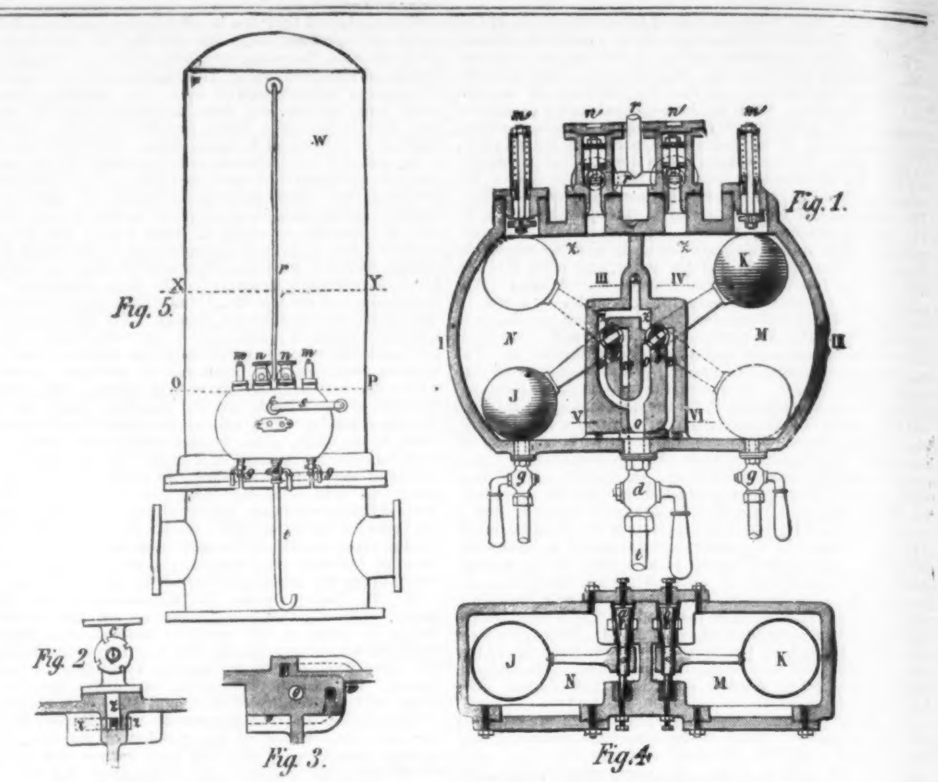
IMPROVED CANAL DREDGER.

We have recently had an opportunity of inspecting a somewhat novel form of dredging machine, erected and worked on the premises of Messrs. Rennie, Blackfriars. This dredger is of a small size and intended for excavating canals in British Guiana—Central America—and is made for the Crown agents of the colonies. As will be seen from our illustration below, the ladder is pivoted on a crane-post at the bow of the ship, and is traversed from side to side as the dredger vessel advances forward in a straight line. It thus cuts the channel the breadth required for the canal, while the vessel only moves in one direction. The vessel is to be made of timber in the colonies, and has a length of 48ft., breadth 13 ft 10 in., draught of water 2 ft. 9 in. The engines are of the two-cylinder vertical description. The ladder is made of iron, and of sufficient length to excavate 9 ft. depth of water. There are seventeen buckets of a capacity of 1¾ cubic feet each, and thirty-three discharge per minute into shoots on either side to discharge on the banks of the canal, and this is further facilitated by a centrifugal pump with a 12 in. fan throwing a large supply of water into the mud. The buckets are made with cast steel backs and links, and work over a top tumbler with ten sides; every other side of the tumbler has steel teeth to catch and pull round the links between the buckets. The result is that the usual thump of the buckets falling on the tumblers is quite obviated, and the bucket chain moves round like a chain over a sheave. The forward motion of the dredger boat and the traversing motion of bucket ladder is effected by an especially-arranged windlass worked by a separate engine, so that either motion may be put in gear at pleasure. The engines for working the buckets as well as for the windlass are furnished with steam from a boiler of ample proportion for burning either coal or wood.

Messrs. Rennie are perhaps the oldest makers of steam dredgers in the country, some of their earlier dredgers worked by steam dating as early as 1806. In these the general arrangement of buckets, ladders, and gearing is much the same as now made, though not of the large size and strength of modern machines. They were worked by a Boulton and Watt steam engine and a wagon boiler built in brickwork. About ten years ago Messrs. Rennie sent a screw propeller dredger of 70 nominal horse power for use in the new harbor of Buenos Ayres, and they are now building a somewhat similar one but of more powerful construction, having buckets of 15 cubic feet capacity each. The vessels are 175 ft. long and 31 ft. beam, and dredge to 32 ft. Five hopper barges, propelled by the screw, to carry 350 tons, or 200 cubic meters capacity, together with a smaller dredger of about half the above size, are being supplied by the same firm. One of the most successful dredgers made is the Teredo for the Bombay Trust.

Dimensions:

Length between perpendiculars	160 ft.
Breadth moulded	29 ft.
Draught of water	7 ft.
Length of bucket ladder, from center to center	79 ft. 6 in.
Number of buckets on ladder	35
Capacity of each bucket	10½ cubic feet.



AIR REGULATOR FOR THE AIR CHAMBERS OF PUMPS.

The machinery is constructed so as to discharge fourteen buckets per minute, and is capable of dredging to a depth of 32 ft.

Engines, compound:

Diameter of cylinders { high pressure	24 in.
{ low pressure	42 in.
Stroke	2 ft. 6in.
Nominal horse-power	70
Nominal revolutions of engines when dredging	36½
Nominal revolutions of engines when using screw propeller	68

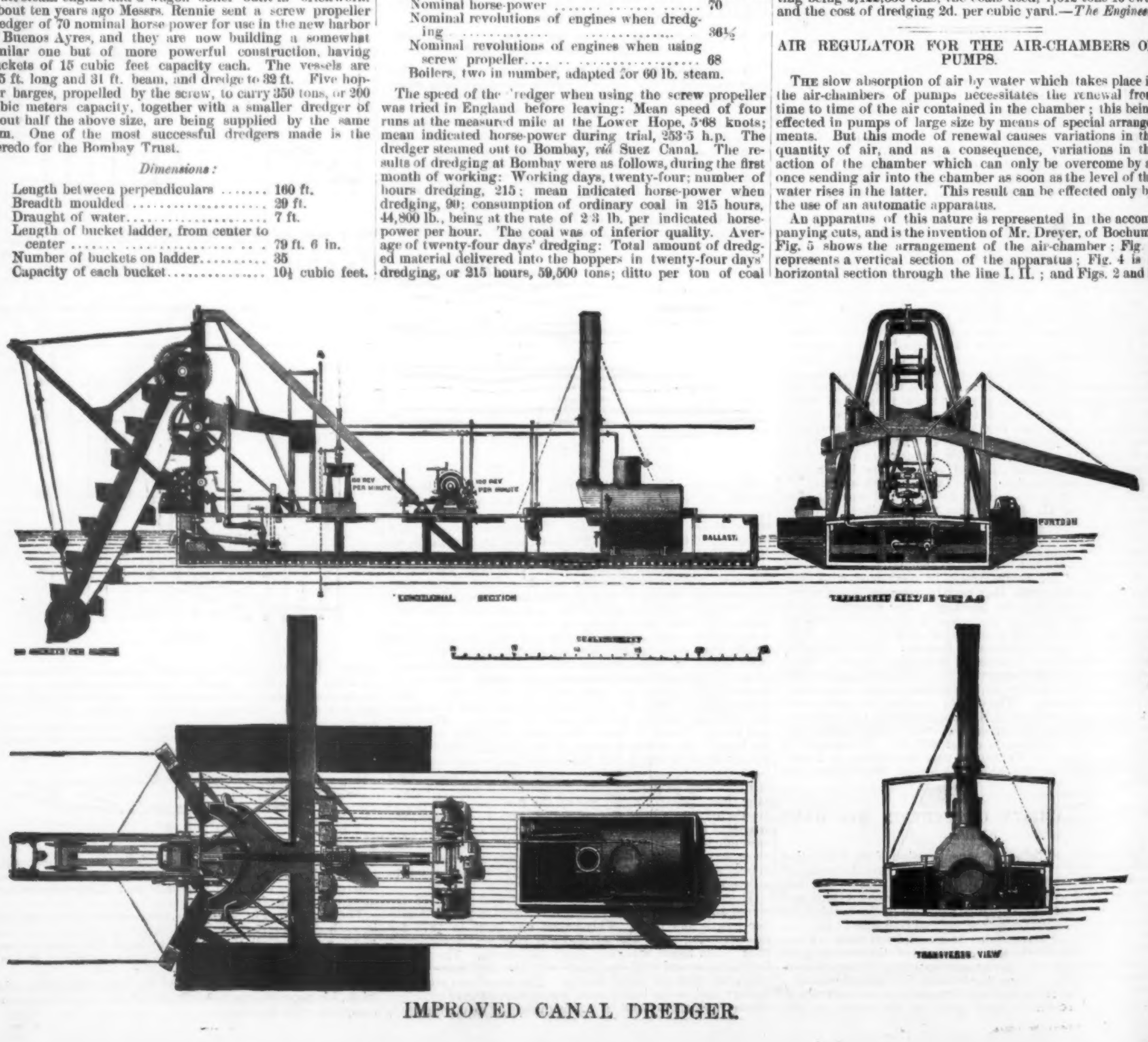
Boilers, two in number, adapted for 60 lb. steam.

The speed of the dredger when using the screw propeller was tried in England before leaving: Mean speed of four runs at the measured mile at the Lower Hope, 5·68 knots; mean indicated horse-power during trial, 253·5 h.p. The dredger steamed out to Bombay, *viâ* Suez Canal. The results of dredging at Bombay were as follows, during the first month of working: Working days, twenty-four; number of hours dredging, 215; mean indicated horse-power when dredging, 90; consumption of ordinary coal in 215 hours, 44,800 lb., being at the rate of 2·3 lb. per indicated horse-power per hour. The coal was of inferior quality. Average of twenty-four days' dredging: Total amount of dredged material delivered into the hoppers in twenty-four days' dredging, or 215 hours, 59,500 tons; ditto per ton of coal consumed, 2,975 tons; ditto per pound of coal consumed, 1·32 tons. Average of two days' delivery into hoppers on 27th and 28th December, 1876: Per day of nine hours, 4,000 tons; per hour, 450 tons. The report of the engineer to the Bombay Port Trust for the month of December, 1876, and for the month of December, 1877, states the work done over thirty-six months' working, the amount lifted out of the cutting being 2,122,350 tons, the coals used, 1,612 tons 16 cwt., and the cost of dredging 2d. per cubic yard.—*The Engineer.*

AIR REGULATOR FOR THE AIR-CHAMBERS OF PUMPS.

The slow absorption of air by water which takes place in the air-chambers of pumps necessitates the renewal from time to time of the air contained in the chamber; this being effected in pumps of large size by means of special arrangements. But this mode of renewal causes variations in the quantity of air, and as a consequence, variations in the action of the chamber which can only be overcome by at once sending air into the chamber as soon as the level of the water rises in the latter. This result can be effected only by the use of an automatic apparatus.

An apparatus of this nature is represented in the accompanying cuts, and is the invention of Mr. Dreyer, of Bochum. Fig. 5 shows the arrangement of the air-chamber; Fig. 1 represents a vertical section of the apparatus; Fig. 4 is a horizontal section through the line I. II.; and Figs. 2 and 3



IMPROVED CANAL DREDGER.

are respectively horizontal sections through the lines, III. IV. and V. VI. The apparatus consists of a box divided by a partition into two compartments, M and N, each of which is provided with a suction valve, *m*, and an escape valve, *n*. A tube. *r*, connects the escape-valve boxes with the upper part of the air chamber, W. In the chamber, N and M, there are two floats, J and K. which are connected with the levers of the cocks, *a* and *b*, and control a system of conduits located in the partition which separates the two chambers. The canal, *v*, leads from the cock, *b*, to the chamber, N; the canal, *w*, from the cock, *a*, to the chamber, M; the bifurcated canal, *o*, from the two cocks, *a* and *b*, to the cock, *d*, of the outlet pipe, *t*; and, finally, the bifurcated canal, *z*, leads from the two cocks, *a* and *b*, to the cock, *c*, of the tube, *s*, which debouches in the lower part of the air-chamber. The apparatus is so arranged with respect to the air-chamber that its upper part or cover shall correspond with the highest level, O P, that the water may reach. Let us suppose, in the first place, that the apparatus is disconnected with the air-chamber and empty, and that the floats, J and K, are consequently at their lowest position. After the water has risen in the reservoir up to a certain level, X Y, the cocks, *c* and *d*, are opened; then the water enters the apparatus through the canal, *z*, and enters through the cock, *b*, and the canal, *v*, into the chamber, N, in which it first compresses the air that it afterwards forces into the reservoir, W, through the valve, *n*. At the same time the float, J, rises gradually, and thus places the cock, *a*, in such a position that the water coming from the air-chamber through the canal, *s*, can enter through the canal, *w*, into the chamber, M, the air of which becomes compressed and is forced into the chamber, W. The float, K, raised by the water, gradually fixes the cock, *b*, in such a position that the canals, *v* and *o*, are put in communication, and that the water which has accumulated in the chamber, N, flows out through the tube, *t*—this chamber becoming filled with air at the same time through the valve, *m*. The float, J, rising in the chamber, N, with the level of the water, fixes the cock, *a*, in such a position that the water from the chamber, M, passes through the canals, *w* and *o*, in the tube, *t*, and flows out. Finally the two floats reach their lowest position, and this action of the apparatus continues as long as the reservoir, W, contains too much water. The two cocks, *g*, serve for allowing any water which may remain in the chambers, M and N, to flow out. This apparatus might well be applied also to the chambers of hydraulic pumps for compressing air, for the purpose of removing the water which is always carried along with the air. Thus the work represented by the volume of water carried along would not be lost, since Mr. Dreyer's apparatus replaces this volume of water by an equal volume of compressed air. The apparatus might likewise serve as a hydraulic air compressor.

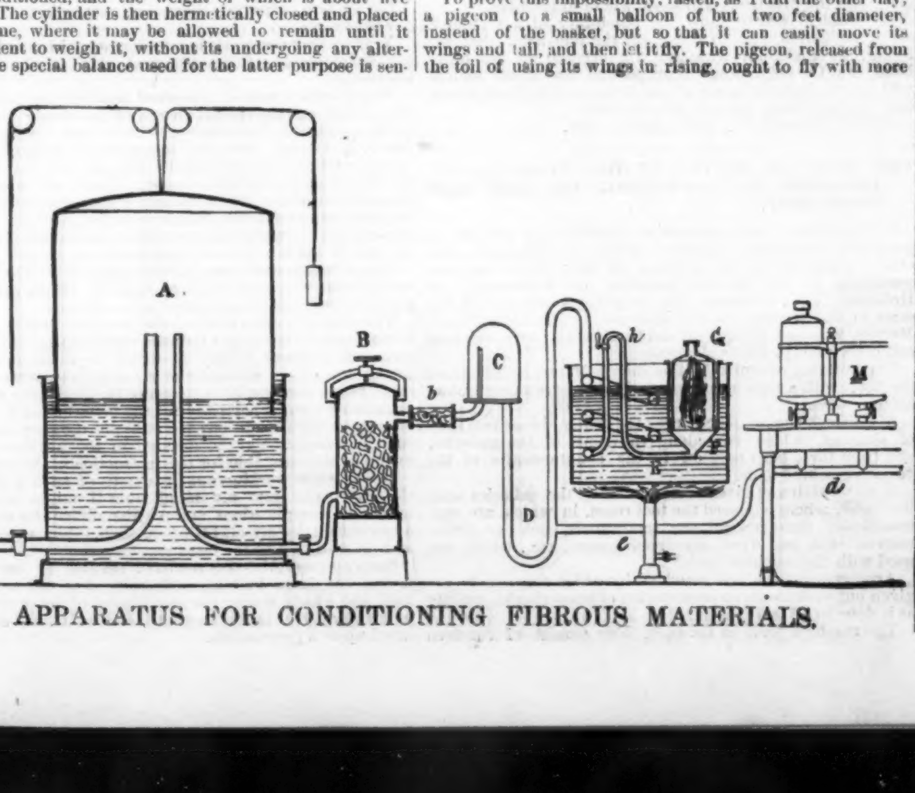
APPARATUS FOR CONDITIONING FIBROUS MATERIALS.

AN apparatus designed for determining the quantity of water contained in woolens and other materials was exhibited at the recent Exhibition of the Wool Industry, at Leipsic, and attracted much attention. This apparatus, the invention of Mr. Hirzel, of Leipsic, is shown in the accompanying figure, one-thirtieth the actual size.

A is a gasometer serving for obtaining a uniform current of air, and which may always be very readily filled with fresh air by simply lifting the reservoir. B is a reservoir lined with lead and filled with pieces of pumice-stone saturated with sulphuric acid, and in which the air rids itself of the aqueous vapor which it contains. *b* is a glass tube filled with caustic potash, which retains the last traces of moisture and the other impurities that are contained in the air. C is a hygrometer, by means of which may be ascertained whether the air is completely dried. E is a bath of paraffine which may be easily heated to 120° or 125° by means of a Bunsen burner, *e*, and in which, at one side, there is a worm having a large number of spirals, and on the other, a variable number of jackets, F, which receive the drying cylinder, G. The worm is connected with the tube, D, which leads the air from the hygrometer and which serves for heating the perfectly dry air to 120° or 125°. From this worm there issue as many tubes as there are jackets, F. These tubes, which are bent, rise above the bath, as seen at *h*, and are provided at this place with stop-cocks; they are afterwards connected with the bottom of the jackets, F, in which they debouch in the form of cones. The cylinders, G, which are of white metal, may be hermetically closed, and are accurately fitted in the jackets, F. When they are opened beneath and inserted in the jackets they locate themselves exactly on the cone forming the extremity of the tube, *h*, in such a way that all the air which arrives through this tube has to traverse the cylinder, G, which is slightly opened above.

When it is desired to use the apparatus, the operator begins by filling the cylinder, G, with the specimen of wool to be conditioned, and the weight of which is about five ounces. The cylinder is then hermetically closed and placed on a frame, where it may be allowed to remain until it is convenient to weigh it, without its undergoing any alteration. The special balance used for the latter purpose is sensitive to one-hundredth of a grain for a load of one pound. The bottom of the cylinder is then opened and the latter is inserted in one of the jackets, F, and the cover of the cylinder is slightly raised and the cock, *h*, is opened. The cylinder, G which is heated externally up to 120° by the action of the paraffine bath, is, in addition, traversed by a current of dry air at a temperature of 120°. The latter acts very quickly and thoroughly, so that in about half an hour the wool in the cylinder, G, is completely dried. Then the cock, *h*, is closed; the cylinder is removed from the jacket, F, and placed on the support, M, in such a way that its lower aperture rests on the cone of the air tube, *d*, which is placed beneath the support, and the air cock of the tube is opened. Dry but unheated air then passes into the cylinder and thus gives, in about half an hour, the cylinder and its contents the temperature of the apartment in which the operation is being performed. Afterwards the cylinder is closed at both top and bottom and reweighed. The loss of weight indicates the quantity of water that was contained in the material tested.

This apparatus is easy of management, and the operation may be conducted in such a way that while a part of the cylinders is in the paraffine bath another part is cooling, a third part is full, and a fourth part is weighed. In ten hours' time, an operator, after practice, can make eighty weighings and condition about 8,800 pounds of wool, with an apparatus having 4 jackets and 8 cylinders.



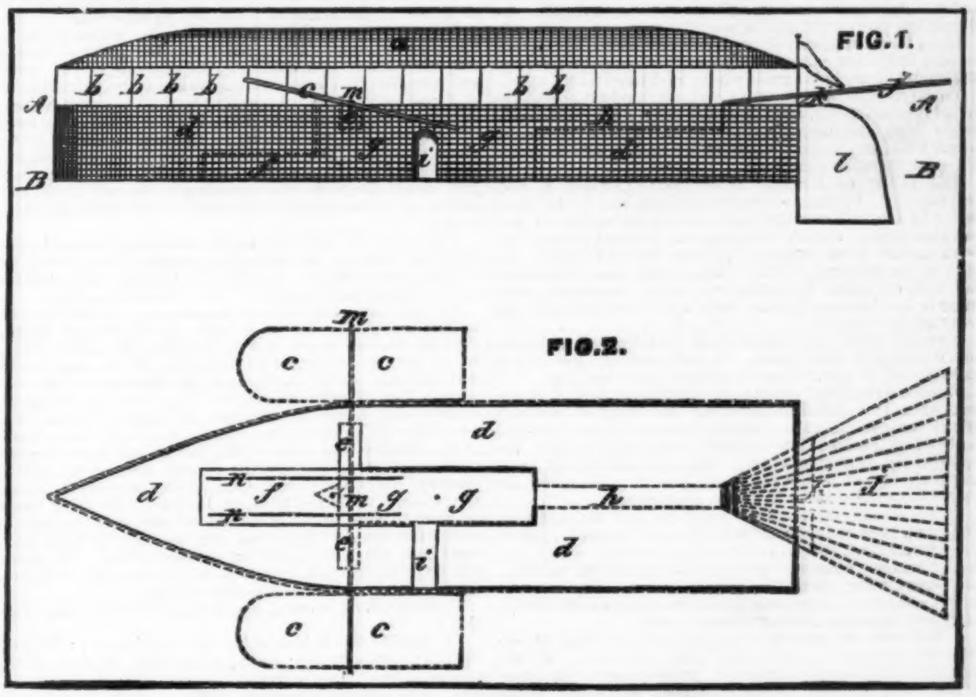
APPARATUS FOR CONDITIONING FIBROUS MATERIALS.

AERIAL NAVIGATION AND ITS POSSIBILITY.

By TIM. CHOINSKI.

MORE than a century has passed since Montgolfier tried his first balloon. Later inventions, as railroads and telegraphs, have reached an almost incredible perfection, but the balloon stands now just where it stood one hundred years ago. Why have aeronauts failed to succeed in so long a time? Is a regulated aerial navigation impossible? It seems almost so after all the failures, but the easy flight of birds shows the contrary. What then is the reason of the constant ill success? My opinion is that men are still too much imbued with old ideas and stick too stubbornly to the wrong shape of the balloon and to the usual propelling power, as if the balloon were a sea ship. Besides, they endeavor to invent a machine so small as to be placed in a nut shell, but powerful enough to propel a cloud against the wind (such is the disproportion of basket to the balloon ball), and this is impossible.

To prove this impossibility, fasten, as I did the other day, a pigeon to a small balloon of but two feet diameter, instead of the basket, but so that it can easily move its wings and tail, and then let it fly. The pigeon, released from the toil of using its wings in rising, ought to fly with more comfort; but you will see it become a powerless plaything to the softest wind; it will flutter without success. The same will be the case with an eagle.



AERIAL NAVIGATION.—T. CHOINSKI.

Is there any man who can build a better flying machine than the pigeon or the eagle? I am sure there is none; and if the eagle is not able to propel a small balloon, no machine whatever can be invented to propel and steer the immense balloon ball out of its little basket. Every trial will be useless and a failure.

To compare the balloon to a sea ship, and to try to propel it on the same or similar principles, is a mistake. The latter, when being propelled against wind, rests on a basis, the water, a more solid body then the adversary; the former is suspended without any basis, is floating in its adversary. How can the same means be successfully applied to the same use in such different situations? The floating without basis is the reason why oars or sails, screws and paddle-wheels—sufficient tools to propel a ship on water—are and must be completely useless and worthless in a balloon.

You will ask, What tools then shall we use to make the balloon fly as we choose?

I answer: Just the same as the birds do in actual flying, that is, NONE.

Everybody was taught in his childhood, every school book says: "*The birds have wings to fly with.*" *This statement*, founded on mistake, proved by nothing, deep rooted in the minds of men, *is wrong*, and is the cause of all failures with the balloon, time having been uselessly wasted upon invention of wings or their substitutes to propel with.

Before I give you the undeniable proof that "*birds have no wings to fly with*," let us first see what flying is. We call vertical motion upward, "*rising*;" the same downward, "*falling or dropping*." The horizontal motion from place to place is actual "*flying*." Sloping motion is a combination of flying and rising or falling, and is differently named according to circumstances.

A stone thrown horizontally or obliquely flies, and yet it has no wings. Why? It does not need them. There is an invisible power that produced its flying. But we shall speak later about it.

Now let us go back to our statement.

1. Put any bird on a table, stretch its wings out, and you will perceive them to be almost horizontal planes, a little inclined down backward, completely unlike oars and unfit to replace them.

2. The birds swing their wings downward and not sideways as rowers do their oars to get forward. With such wings and such swinging of them, no bird can be able to propel its body one foot forward even in the quietest air, and the less so against wind and storm.

3. If flying would be produced by striking of wings, then those birds that in a given time swing their wings the quickest, would fly fastest; hence the unwieldy poultry would be swifter than the swallow, which makes in one minute not half as many strokes as the flying hen in the same time; and

4. Birds would have to swing them incessantly, especially when flying against wind. But we see just the best fliers, namely, eagles, gulls, and swallows, fly long distances with outstretched but motionless wings.

Besides, the physical strength of no bird is sufficient to vanquish the power of a storm, and yet we see birds flying against storms.

Wings are, as we see, not the means of flying. THIS IS NOTHING BUT THE ALMOST HORIZONTAL FALLING OF BODIES, PRODUCED AND SUSTAINED BY LAWS OF GRAVITATION, AND STEERED BY WINGS AND TAIL AFTER THE LAWS OF INCLINED PLANES.

We have to discern in each body its two important points: the center of its bulk and the center of gravity. Both are seldom joined in the same spot. A straight line connecting both will show the direction of fall of the body.

Is this line vertical and supported, then the body rests; if not supported, it falls vertically.

If the connecting line is not vertical, the center of gravity endeavors to get in such position, tilts the body if not hindered, till it falls perpendicularly under the center of the bulk.

The farther the center of gravity lies from a vertical line running through the center of the bulk, the greater is its pressure, according to the laws of the lever.

The center of gravity being supported and thus hindered in getting into the vertical line, the body glides—falls—

in the direction of the connecting line; it moves sidewise—it flies.

Supposing the center of gravity to be the fore part, the center of the bulk the back part; if the former lies higher than the latter, the body will glide backward.

If the means supporting the center of gravity is also exposed to fall, the flying of the body is regulated by the laws of the parallelogram of forces.

The farther the center of gravity lies from the vertical through the center of the bulk, and the slower the fall of the supporter, the more horizontal will be the fall, the flying

According to the laws of gravitation the velocity of the fall increases every second. It is after the first 32 feet, after the second 64 feet, etc. Velocity produces and represents power, and it is conspicuous that a body falling after the third second with the velocity and power of 96 feet will fly against and pierce even the greatest storm of 40 miles velocity to the hour, or about 60 feet to the second; farther that *this fall power is the only means to make flying possible.*

I have dwelt on the above laws so long and explained them particularly, because corresponding to them the bodies of birds are shaped, and *their flying is based on them alone.*

Let us now look at the birds' bodies and consider the functions of their single parts.

The body is surrounded by thick plumage, to make its specific weight smaller and the bird fitter for rising. Legs and feet are supporters of the body in resting and walking, but of no use in flying. The beak, head, and, in flying, extended neck, are small, slender, to be of the least resistance to the air. The shape of the body itself is oblong, tapering toward the back part. The center of gravity lies in the forepart of the breast, where the most flesh is accumulated, not in the center of the bulk. The wings when spread out serve—1. To rise with. 2. To support the center of gravity. 3. As a parachute to lessen the vertical fall. 4. To steer with sideways. 5. To regulate the flight. 6. To stop with, but *never to propel with.* At last the tail is not a rudder as we are taught of, in which case it ought to be vertical and not horizontal, but a simple mechanism to regulate the flying line downward and to poise the body.

Good fliers, like birds of prey, gulls, swallows, have a long slender body, low sternum, the center of gravity far from the center of the bulk, great wings, and large tails.

Poultry, especially chickens and all kindred species, as partridges and quails, have a rounder body, a more spheroidal shape, high sternum, the center of gravity near the center of the bulk, and proportionally rather small wings and tails. They are therefore poor fliers.

The rising of birds is three fold: 1. From a resting place; 2. In common moderate flight; and 3. In rapid fall. In the first case they turn against the current of air, raise the breast, stretch the wings to inclined planes, like boys do with kites, and push off with feet and wings. As soon as the intended height is gained, they make themselves fall with outspread but motionless wings, regulating their more or less horizontal flying with their tails, and poise the body by the latter.

You may prove it by keeping any bird in hand and turning it suddenly head down. It will instantly spread and raise its tail, to prevent, in its opinion, a vertical fall.

When a flying bird wants to go sidewise or turn, it slopes backward to an inclined plane but one wing of that side where it wants to go to; when it wants to rise, it slopes both wings backward to inclined planes and glides on the opposed air, as if on a wedge. Such exercises in full speed but with almost motionless wings you may see gulls and swallows perform at summer time above water.

In the second case, when birds are in full flight of long distances, they swing their wings almost regularly, not to propel with, but just to interrupt the velocity, which else would become immense; and to make the flying uniform, the wings being used like pendulums; at last to regain and rise the few feet the body has fallen in order to be always in almost the same distance from the earth.

A bird stops its flight by raising its breast as high as possible and sloping the outspread wings backward. By the first action it raises the center of gravity above the center of the bulk, and the falling line goes backward; by the second, two planes are opposed to air and wind—both good means to check flying instantly.

We have farther to mention that, because winds and storms do not blow continuously but in puffs and squalls, an interval of but few seconds may be sufficient to give the flying bird the necessary time to get the required direction and velocity to pierce even a hurricane.

At last that, except when resting in an air-quiet place, the birds flying even in a calm atmosphere have, according to their velocity to deal but with wind and storm, just in the same way as we perceive such relative wind, when going on a train in exactly quiet air. Wind and storm are therefore of no influence on birds. These perform the necessary movements with the rapidity of a thought by mere instinct, as we move our limbs.

The above details show, I hope, sufficiently that birds neither have, nor need any means to fly with; consequently, *that to fly no machinery whatever is necessary;* farther, that an air ship or balloon must be almost of the bird's shape and not consist of two parts—ball and basket—separately, as we are used to see it; at last, that, to fly in any direction we choose, the air ship has to perform just the same movements as birds do.

The long-looked for power—an immense one—to be produced by no fragile machines, but an agent of nature itself, the fall power (gravitation) is given; the means and ways of steering are shown by birds; we need nothing more but courage and experience. There is plenty of the former in our United States, the latter we shall acquire in the course of time.

Let us now consider how the shape and the abilities of birds can be replaced on an air ship:

1. The shape must be of a skiff, but with higher and vertical side planks.

2. The small specific weight of a bird produced by its feathery envelope is to be paralleled by surrounding the ship with hydrogen.

3. The center of gravity in the forepart of the bird's breast is to be secured by locating persons, more especially goods, in the front part of the ship, off the center of the bulk.

4. The ability to raise the center of gravity in starting and stopping, by an arrangement to bring persons and goods close to the center of the bulk. (Goods may be put in a basket-wagon running on wood rails.)

5. Wings, and their force to rise with, to be substituted in starting by the gas envelope; in flying, by throwing ballast overboard.

6. Wings, as supporters of the center of gravity, as parachute, as rudder to steer, and as inclined planes to stop with; replaced also by wings of corresponding size, revolving on an axle above the connecting line of both centers. (According to my unpresuming opinion, good fliers have their wings nearer the center of the bulk, and to poise the body with equal ease, therefore longer tails.) The wings of the air ship must be independent from each other, and fit to be fastened in any position. A flat bottom to the ship would assist the wings in keeping the ship from vertical fall.

7. The bird's tail, which regulates the flying line down, and prevents the bird from tilting head foremost, to be substituted the same way by a tail, pliable like a fan, and movable up and down.

8. The bird's ability to make itself fall to be produced by discharging gas out of the envelope. This usage ought to be hereafter replaced by condensers—force pumps.

Besides, we have to add that, wings and tails being outside the gasy envelope, an easy access from the ship to them must be provided for.

To protect persons and goods against sunbeams and rain, some cover would be necessary. This could be of great help if made half oval, filled once for all with hydrogen, and fastened to the ship with withes, to make but one body, the distance between cover and ship being wide enough to allow the current of air to act upon the tail.

That a compass and sextant would be needed is a matter of course; also, that the center of the ship's bulk ought to be marked.

There would be no need to rise with the air ship thousands of feet high, except we hoped to get into a fairer current of air, or to get space for a very long falling line, and herewith the necessary velocity to overcome an opposing storm.

At any rate the first experiments should be made in a quiet atmosphere, to avoid, in starting, greater impediments and difficulties. May be it would be of good use to apply a helm to the ship just in the beginning, before men can get experience in managing it right and quick.

Thus far about the arrangement of the air ship. Now let us go over to the action of rising and flying itself.

Before starting we have to consider the action of the atmosphere, especially if we can reach the place of destination straightways (in calm air, in currents of the same, and opposite direction), or after the law of the parallelogram of forces (side wind). According to this statement we have to turn the ship and fix the helm, if there is any, then make the ship rise, and when rising to endeavor to keep it in the given position. The wings are to be loose and hanging vertically, to avoid any unnecessary resistance. If the back part should rise more, the spreading of the tail, or the concentration of the weight to the center of the bulk, would impede it.

Arrived to the required height, the wings are to be fastened horizontally, the helm, now useless, to be loosed, the ship inclined a little to the front (by bringing hither a little more weight), and brought to fall by discharging gas.

The air ship when rising in a current of air will be carried along, but as the experience shows, without feeling even the heaviest storm, and is, therefore, in a relative rest. In this way the current does not exist for the ship, which will fall when caused to do so, in the direction of the connecting line of both centers, and in a few seconds get a velocity and power to vanquish the wind or storm. The perception of an increasing current of air will be the result of the velocity, and show us the fact of our flying the proposed way. If by the blowing current our ship was driven sideways out of the course, then having reached the necessary velocity, we have to slope backward the wing where we want to turn, to get in a diagonal course to the place of our destination. If fallen too low, we have to slope backward both wings, as to rise on inclined planes. Stopping will be produced by fastening the wings vertically, and pushing the weight to the back part of the ship.

To illustrate my words, I give a sketch of an air ship of my idea

The outside dimensions of the gas envelope are: Length, 100 feet; width, 25 feet; height of the hull, 10 feet; of the cupola, 5 feet. Inside basket's length, 30 feet; width, 7½ feet. The hull contains about 12,000 cubic feet; the cupola 5,000 cubic feet; a space for gas. Bearing capacity is 1,400 lb. (of it 500 lb. for material, 300 lb. for ballast or goods, and 600 lb. for four men—the crew—viz., the captain, one man to the tail, and two for the wings—one to each. Basket and frame to the envelope made of willow work, surrounded by air-tight stuff and net, like a common balloon. The sharp, pointed forepart of the hull to be covered with light tin and iron sheets. The wings 30 to 40 feet long, 10 feet wide; the tail 30 feet long, supported by a movable bridge, as its mobility downward will not be required.

This sketch of mine shall not be peremptory, not being a mechanic; I omit even the means and ways of fastening wings and tail. All details I leave to experienced technologists, and the first trials will show where to improve.

My problem was to show the possibility of a regulated aerial navigation, and to prove, on the one hand, that, for flying, no machines whatever are needed; on the other hand, that the only means to propel and steer an air ship at will are *the immense power of gravitation and inclined planes, both, and not wings, used in flying by birds.*

Warren, Pulaski Co., Ark., March, 1881.

THE STEVENS INSTITUTE OF TECHNOLOGY, HOBOKEN, N. J.—OPENING OF THE NEW WORKSHOP.

In response to an appropriate invitation, a number of prominent engineers, members of the scientific press, and others, assembled on the evening of May 14, in the new workshop of the Stevens Institute of Technology, at Hoboken, N. J., to witness the formal presentation of the same to the trustees of the Institute by President Henry Morton, who had fitted it up and furnished it with machine and other tools at his own expense.

The building occupied by this shop is 50 feet by 80 feet on the floor, with a high open roof, and galleries running around all four sides.

A Buckeye engine, placed near the center, drives two lines of shafting, which run along the fronts of the galleries, and from these belts pass off to the counter-shafts of the various machine tools.

A spiral stairway gives access to one of the galleries near its center, where is placed the tool room, in which are systematically arranged all the small tools, such as drills, cutters, taps, and dyes, mandrels, gauges, etc., which are used with the machine tools.

Arrangements are here provided by which these tools are given out to students on presentation of brass checks, exactly as is done in all large shops.

The machine tools on the main floor consist of fourteen engine lathes of different sizes, from one of 22 inch swing and 9 foot bed downwards, all by different makers, and thus presenting a wide range of variation in style and structure; two planers, with beds 20 inches by 5 feet; two drill presses; and one universal milling machine. There are, besides, grindstones and emery wheels driven by power, and a large number of vises, work benches, sets of wood working tools, and all other accessories.

It was in this building that the visitors assembled on the above occasion, the space being brilliantly illuminated by the combined effect of electric and gas lights.

The proceedings were opened by President Morton, who delivered the following address:

ADDRESS BY PRESIDENT HENRY MORTON.

Mr. President of the Board of Trustees, and Gentlemen who have honored us by your presence this evening:

At the present time, a brief historical review of the development of the mechanical department of the institute will, I think, be very much in place.

The policy of the trustees of the Stevens Institute of Technology throughout, has been one which is capable of illustration by an analogy taken from the realm of nature, rather than that of art. They have endeavored to plant good seed and to judiciously foster its growth, rather than to erect an edifice whose future should be strictly limited by the conditions of its first construction.

Thus at the very outset they selected a faculty of young men, whose reputation in the world of science to a great extent was yet to be made, and whose life work was essentially before rather than behind them, and so placed the organization and development of the institute in their hands, that its future would of necessity be their work, and they and it could harmoniously develop together.

The wisdom of the selection, and of the free scope for development offered, is seen in the fact that no similar school can show a more distinguished list of names in its several departments, or can present such a catalogue of original investigations and contributions to scientific and technical literature as have emanated from the Stevens Institute of Technology.

In the special department of mechanical engineering the history of the institute has been likewise a history of growth.

The field was essentially a new one, and it was only by experience that it could be shown how much of practical work could be effectively carried on, together with the extended theoretical training which it was and is the chief object of this institution to afford.

Our object always has been and is, to graduate, not journeymen mechanics but mechanical engineers, and the long list of our graduates now occupying high positions of responsibility in the various machine shops of the country, bears abundant witness to our success in the past. For the future we have no idea of allowing our workshop course, in any way, to displace the invaluable instructions of the other departments, but on the contrary, we intend that it shall render them only more efficient, by making closer their relations to what every student sees to be the object of his course here, namely, the acquirement of the various and extensive knowledge, scientific, mathematical, and practical, which will enable him to grapple successfully with the vast and difficult problems daily presented to the mechanical engineer.

To master such problems he must not only be practically familiar with the operation of machine and other tools, the process of moulding and forging metals and the like, but he must also be able to understand at a glance the ideas of others as expressed in "mechanical drawings," and express his own ideas accurately in the same way.

He must also have a complete mastery of all mathematical processes available for calculating the action of forces, distribution of strains, transformations of energy, and the like.

He must likewise have a large acquaintance with the vast body of recorded experience and logical deduction from the same, which constitutes the science of mechanical engineering.

He must also have such a knowledge of the facts and laws of physics and chemistry as will enable him to employ the forces of nature here indicated for his purposes, and avoid their inimical influences.

Yet, again, he must have such a knowledge of modern languages and of history, literature, and the other elements of social culture, as will fit him to associate on terms of equality with other educated men.

Lastly, but not least, he must have such knowledge of the financial relations of his subject, the cost of labor and material, the relative economy of various processes and the like, as will enable him to choose judiciously in selecting an outfit for any mechanical establishment, and estimate accurately as to its cost.

Let me now, after this digression, return to the history of our workshop.

Year by year our workshop has grown with a corresponding development of the practical side of the course of instruction.

This growth received a marked acceleration, when more than a year ago the special charge of the shops was placed by the trustees in the hands of one of our own graduates, Mr. J. E. Denton, who had distinguished himself not less by his marked capacity than by his zeal for, and devotion to, the interests of his *alma mater*. Under his energetic and unremittent efforts, the workshop course was so developed that the accommodation which, year by year, has been becoming more straightened, was felt to be already, or sure to become in the near future, manifestly inadequate.

Under these conditions, various plans were discussed, by which some provision might be made to afford such workshop accommodation as seemed to be required.

The original endowment of the institute, while sufficient to maintain its running expenses, could not be called upon for such a heavy outlay as would be demanded for the fitting up of an extensive new shop; and it was, among other plans, contemplated to secure the necessary funds, by organizing a stock company for the manufacture of machinery, under such conditions as would put the shops erected by such an association at the disposal of the institute, to such extent as was desirable for the instruction of the students.

It was while working out the details of such a plan that the idea came into my mind, that I might escape the numerous complications and possible difficulties of such an arrangement, by myself fitting up such a shop as was needed and presenting it to the institute.

Such an enterprise was rendered feasible by the possible use of the large building originally designed as a lecture hall, and which, during the early years of the institute, was eminently useful in that capacity, and which had since been fitted up as a gymnasium.

This plan having been approved by the trustees, and the large cost of erecting a new building having been avoided, I have been able, for the moderate outlay of $9,500, to fit up this workshop and stock it with machinery and tools as you see it. There are some tools and other appliances requisite for carrying out our contemplated course of instruction, which have been ordered and are in course of construction, but are not yet in our hands; but as nearly as we can estimate, the entire cost will not exceed $10,000, and if it does, I will see that what is needed is furnished from the same source as the rest.

In conclusion, allow me to hand you this memorandum and package of vouchers, which represent the amounts already expended. These may be briefly summed up, in round numbers,, as follows:

Carpenter's, mason's, plumber's, steam fitter's, and painter's work in building galleries, opening windows, building piers, making lockers, cases, etc., about	$3,100.00
Shafting hangers, pulleys, belting, and labor in placing the same and in setting up the machine tools, about	1,100.00
Steam engine and machine tools, including chucks, about	4,800.00
Small tools, gauges, cutters, etc., about	500.00
	$9,500.00

Allow me to present to you at this time, on behalf of the American Steam Gauge Company, who have generously donated it, the beautiful "indicator" which you see attached to the steam engine.

Finally let me take this occasion to express my high appreciation of the unvarying and kind sympathy with which I have always been sustained and encouraged by yourself and the other members of the Board of Trustees, and my conviction that, whatever has been or may be accomplished in this institution, will be primarily due to your large minded and judicious management.

To the above address the Rev. S. B. Dod, President of the Board of Trustees, replied as follows:

ADDRESS OF REV. S. B. DOD.

It becomes my pleasant duty, on behalf of the trustees, to accept the generous gift of the President of our Institute, which does honor alike to his intelligent appreciation of the wants of the students, and his hearty interest in their welfare.

To the thoughtful interest which he has manifested from the very beginning, the Stevens Institute owes its success. He has been ably seconded by a corps of professors, heartily in sympathy with his plans, and thoroughly conscientious in maintaining each his own department fully abreast of the times.

The best evidence of the success of an institution of learning is in the kind of young men which it turns out, to take their place in the great world.

Our graduates have borne splendid testimony to the men who have trained their minds and their hands to work. Wherever they have gone, they have made their mark. There is an air of serious earnestness about them in their course of study, that shows, even to the casual observer, that they are here for work; and when they go out from us, they show that they have been trained to work with head and hands. And the evidence is before us in the graduate who has charge of this shop, and of the connected department of experimental mechanics, and in that good work which distinguishes those who study under these able and earnest teachers.

We accept, therefore, this gift from President Morton, with our heartfelt thanks for the generous spirit that prompted the giver, with the hope and belief that it will realize for the young men who study here all the benefits that he hopes to realize, and with the assurance that we see in this only another evidence of his hearty devotion to the welfare of the Stevens Institute, to which he has ever given that which is worth more than all else, the earnest, thoughtful, intelligent purpose to make this institute a success.

Next followed an address by Mr. Coleman Sellers, of Philadelphia, who, as a representative of the Mechanical Engineers of the country, spoke as follows:

ADDRESS OF COLEMAN SELLERS, M.E.

From the very first moment when it became known that the liberal bequest of Mr. Stevens was to be used as a means of educating mechanical engineers, I have felt an interest in the success of the effort, for reasons that will be understood when I tell how I came to know more about it than is usually the case with outsiders.

A good many years ago (how many I do not like to say) it was my good fortune to make the acquaintance of a young man in Philadelphia, who, at that time fresh from college, was engaged in teaching in one of the schools. He showed great aptness in grasping knowledge, and could, with remarkable cleverness, make others the recipients of what he knew. Beside being skilled in the use of his pencil, he was a good workman in metal and in wood, and I came to know him through his making some of the pieces of apparatus he needed in his teaching. From him I found that I could obtain much that was of use to me, and he, in turn, asked my advice in mechanical matters; so in a short time there grew up a warm friendship, fostered by kindred tastes.

When the Franklin Institute of the State of Pennsylvania came to select a permanent secretary, who should be, as it were, the scientific head of that institution, it was so fortunate as to secure the services of this young man, who, in an admirable manner, carried out the plans inaugurated at the creation of this office. Brilliant lectures to crowded audiences drew attention to the institute and its work, and added to the fame of the lecturer. Soon he was made editor of the journal of the institute; then he was granted leave to fill, for a time, one of the most important chairs in the University of Pennsylvania.

His connection with the Franklin Institute brought him in contact with the leading manufacturers of the City of Philadelphia, and developed in him a fondness for the profession of engineering in its broadest sense.

The trustees of the Stevens bequest saw in him a means of judiciously utilizing the money they had to expend, and thus my friend, Prof. Henry Morton, came to be made the President of the Stevens Institute of Technology; and although I do not see him daily, as was my pleasure in the old times, yet still have I known of all his work, and I rejoice in his growing success.

I think that his effort in turning the educational scheme in the Stevens Institute, in the direction of the training of mechanical engineers, grew out of his familiarity with their needs in his connection with them in the City of Philadelphia.

There is one direction in which I have always found him particularly strong, and that is in his caution in conducting experiments and in his careful selection of methods. He has always looked upon this scheme of educating mechanics as one that must be tried in such a way as to make each step in the process of experimentation a step in advance. He tells you what he has had in view, and he calls on me to say what I think of the plan—a plan to carry out which he himself has, with commendable liberality, furnished the wherewithal.

Measured by his own pecuniary ability to make such a gift to the world as he now conveys to the keeping of the trustees of the Stevens Institute, it is a truly munificent gift. Measured by the results that are likely to be attained by its use, it represents a still greater value. That such results will be reached, we have every reason to expect, for this is the outgrowth of what has been of use in a smaller scale, and it presents a possible elasticity that will make it bend to what is found to be of the most value, or what will produce the best results as the experiment progresses.

A few years ago the impression obtained among teachers that education must be directed to the training of the mind only, and but little effort was made to make the hands take any part in the system save in the one thing of using the right hand as the guiding member in writing. The common schools and the colleges too, turned out boys ready to barter, or may be to become members of some of the learned professions, as they are called, say doctors of medicine, lawyers, preachers, and the like, but not to be mechanics, and I dare say the idea of a machinist requiring a more extended general knowledge than the doctors, seldom entered the heads of those who should have given thought to the subject. To become a good journeyman machinist requires that there shall have been a thorough training in the art, and constant practice. His skill comes from application, and may be separate from any great amount of mental training. That is to say, the minimum amount of book learning may serve the purpose of any one who aims to become a skilled artisan only so far as his hand training is concerned. To be a mechanical engineer is a very different matter indeed. A learned professor, once speaking of a certain mechanical engineer, and intending to compliment him, said that he believed him to be a distinguished *amateur* physicist. Now it seemed an odd thing to call that man an amateur whose whole success in his profession came from his thorough knowledge of the laws of physics, whose everyday tools were those laws—who to do what he had to do in his everyday work must have at his very finger ends, as it were, all the learning of many very learned professors, and if he cannot keep all this vast amount of knowledge in constant working order, he must at least have such an acquaintance with books and their contents as will enable him to go at once to the fountains from which he must draw his supply of knowledge.

The mechanical engineer who has grown up through the shops only, without any preliminary training in the schools, has a very hard road to travel; hence many who rank high in the profession wear themselves out in the effort to educate themselves up to the requirements of the times.

When the idea first dawned on our educators that some effort must be made to teach those who would be machinists, inasmuch as there was but little chance for all who wanted to learn, to got into the shops; the problem seemed easy enough of solution. We had but to add the required shops to our schools, and the thing would be done.

Adding shop practice to the regular school course, *did not do it*, and for the very simple reason, that no one can make a skilled mechanic, in the sense that one is so rated in the shops, in so short a time as say 1,000 or 2,000 hours, and that is about all the time that can be spared from a three years' course in a general college education. The shop, too, was shown to be a very expensive adjunct to the school; if it does not produce salable material, it expends large sums of money in the process of teaching. School shops then began to compete with the other workshops of the land as producers, but it took a very little time to convince those who first tried the experiment, that raw boys cannot be made to do work that will sell in competition with the work of the well organized and well equipped manufacturing establishments of the land. So now one can find, without searching very far, some such shops idle.

After many failures of this kind, there came a new system into vogue that has been called the Russian system, most excellent in its way, by means of which skilled workmen are trained in a shorter time than by any method with which I am familiar, but which, of itself, will not do all that is wanted.

The Russian system seeks to *instruct* without trying to *construct*. That is to say, by a well selected series of manual exercises, the hand of the pupil is trained to do certain work, while he is not hampered by the fear of loss of material worked on; this scheme of training permits a graded marking as to proficiency, which is as readily applied as in any other school exercise. We may accept the Russian system as a step a long way in advance in the training of skilled artisans, and it is likely that the introduction of that system into our workshops, and the seeming loss of the apprentices' time during the period of instruction, may, in the end, be found to be more than compensated by the superior skill developed by systematic training, in comparison with the process of learning as best he can, now in vogue in shop training. But the young man who aims to be a master-mechanic needs much more than he can get in the workshop or in the school; and to acquire all that is needed, he wants time in the drawing room, in the shops, and in the office. The latter plays an important part in the shop economy. Let a man be ever so good a mechanic, if he be not also a merchant, he is lacking in what makes the difference between success and failure. The great question involved in all engineering work is, "Will it pay?" To make a machine work is one thing; to make it work without costing too much, is quite another matter. Here is where I look for the great results from this effort of the Stevens Institute to still further develop its capabilities in the direction of training mechanical engineers. As I understand the intention of President Morton in the use of this shop as a means of education, it is to have the money or cost element fully developed, and in this I think it will make a long step in advance. I have carefully considered all the problems involved in this scheme of teaching, and cannot but predict the happiest results.

President Morton's gift is not to the Stevens Institute alone—it is to the world; and it behooves those who have the interest of the rising generation at heart to aid in all ways possible in the success of this enterprise. As one of the mechanics of America, I thank President Morton for his gift, rejoicing that another door has been opened for those who would add to our country's prosperity by aiding in the increase of her production. For it is to education well applied only that we must look for continued progress in competition with the nations of the world.

THE FUTURE DEVELOPMENT OF ELECTRICAL APPLIANCES.*

By PROFESSOR JOHN PERRY, B.E., Assoc. M.I.C.E.

IT had been my intention to introduce this subject to your notice by speaking of the great things physical science has already done for humanity. I assure you, that I had arranged a most effective harangue on this subject, touching on the Bacons, and Newton, and Boyle, and Watt, and Faraday, and Joule, and Thomson, showing that it was these men in their laboratories who opened the way for Stephenson, Wheatstone and Cooke, Gramme, Hughes, Edison, and Graham Bell. I meant to tell you how, in days gone by, a few Birmingham business men subscribed to give their townsman, Priestley, sufficient money to live upon while working at original research; and I felt able to prove so clearly to you, that it was for the good of the nation to provide scientific men with large laboratories, and to insure them freedom from ordinary cares, that in the mere preface to my proper subject, I prepared an hour's lecture. Luckily, I remembered that you had all had opportunities of hearing about the benefits you owe to science; and I bethought me that you might even be tired of listening to truisms regarding endowment of research—truisms to members of the Society of Arts, but not so well believed in by the general public, and especially by that section of the general public which sees reason to lean on Mr. Ruskin, in whose nostrils the mere names of Watt and Stephenson are as the savory odors of the Thames at low water, and attends to the views of Sir John Ellesmere, who hated telegrams more than he disliked our common enemy. Men of this stamp may well think of the future with horror, for there is every sign that applied science is increasing the acceleration of the rate of its development. To such men I would say: Put a stop to laboratory work; set your faces against the endowment of research; root up the acorn if you would not in the future be plagued with the oak. The applied science of the future lies invisible and small in the operations of the men who work at pure chemistry and physics These men do not know what will be the outcome of their labors. They often think that they sympathize with Mr. Ruskin; but you might as well ask a dram-drinker to give up that which his soul loveth, as ask a man who has done real experimental work to give it up. I have often watched Sir William Thomson, to whom every object in nature is continually suggesting ideas, new experiments; to whom every particle of brass scraped off by a file is a being full of complication, an object of interest and a thing of beauty, and to whom the study of the bending of a bit of brass wire is a joy for ever. Sir William Thomson believes in applied science, but such belief has really nothing to do with the delight which he and every other experimenter has in his work.

Now, electrical science has reached a position from which, on every side, hundreds of enticing paths lead forward into unexplored regions of nature. At every step in advance, the laboratory worker sees to right and left of him new and promising lines of research; and he feels that, for the work to be done, the present army of explorers is all too small and weak. But interesting as it might be to prophecy on investigations newly begun, it is rather my purpose, to-night, to take you upon the well-trodden ground prepared for us by Faraday and Joule and Thomson, to show you how, in one or two great lines of the applied science of electricity, certain fixed laws tell us about the future. I shall then speak of a few of the more recent discoveries.

Now, in the first place, you must remember that electricity is, to us, something that can be measured; although, unfortunately, to the ordinary telegraph operator, this is not the case. If you can imagine a mechanical engineer regarding a distance of a few inches as being equal to the distance of a few miles, or even of a few thousand miles; if you can imagine a grocer to confound an ounce of sugar with a ship's-load of the same material, you get a too truthful idea of the vagueness, the general want of definiteness, in the notions of nearly all students of this subject until a few years ago, and, I am sorry to say, that much of this vagueness is still to be found even in modern scientific papers. Perhaps, when electricity is supplied to every house in the city of London at a certain price per horse-power, and is used by private individuals for many different purposes, this vagueness will finally disappear.

To get exact ideas in any department of physics, we have one firm foundation to build upon, viz., that a certain amount of energy or power of doing work remains always the same, in whatever form it may appear I have here various sources of electricity—a voltaic cell, a thermopile, a glass-plate machine, a magneto-electric machine, which may be turned by hand, and two dynamo electric machines outside, which I can drive by means of a steam-engine. As you know, there are many others. To all these some form of energy is given, and they convert this energy, badly or well, into electric energy. The cell burns zinc; in the thermopile gas is burnt; to the three last machines mechanical energy is given; they all give out electrical energy. Now, how do we know that there is a production of electrical energy? Let us take any one of them (this voltaic cell, for instance). Some form of energy is given out, for you see that I can convert it into heat. (Experiment shown.) Here I take advantage of a property somewhat analogous to mechanical friction.

This thermopile is also generating electricity. To test this I connect its poles to the wire of a galvanometer, and the instantaneous deflection of the needle of the galvanometer tells me about the current. (Experiment shown.) Here is another proof that some kind of energy is traversing the wire connecting these two screws. The two wires are attached to an arrangement at the other end of the room; when I complete the circuits, whether I do it here or there, the bell rings. (Experiment shown.) You see that in this case the heat energy given out by this burning gas is converted partly into electrical energy, in which state it can be transmitted to a considerable distance, and there converted into mechanical energy, or into sound, or into any other form of energy. In these and other ways we can detect the existence of the electrical energy coming from all these generators, and measure its amount. Now, Joule's experi-

* A paper recently read before the Society of Arts, London.

ments tell us that any generator gives out exactly as much energy as is given to it, but much appears in the form of heat. All these generators get heated, and may be said, therefore, to waste energy. One great object of the inventors of such machines is to give out as much as possible of the energy supplied to them in the shape of electrical energy. You must clearly distinguish between electricity and electrical energy. A miller does not merely speak of the quantity of water in his mill-dam; he has also to consider the height through which it can fall. A weight of one thousand pounds falling through a distance of one inch represents the same energy, that is, gives out the same amount of work in falling as one pound through one thousand inches. A mere statement, then, of the quantity of electricity given out by a machine is sufficient; it is also necessary to state what is the height or difference of potential through which it is falling. The quantity of electricity in a thunder cloud is comparatively small, but the difference of potential through which this quantity passes when discharge occurs is exceedingly great. So it is with the two factors of the electrical energy developed by this glass machine. The quantity of electricity obtainable from this machine is comparatively small, but it is like a small quantity of water at an exceedingly great height, whereas, in all these other machines we have, in the analogy of the miller, a very great quantity of water and a very small difference of level. I put this water analogy before you because you have all more or less exact notions about water, and because, within certain limits, the analogy is a very true one. I have traced it more fully in the wall-sheet I.

WALL-SHEET I.

We Want to Use Water.	*We Want to Use Electricity.*
1. Steam pump burns coal and lifts water to a higher level.	1. Generator burns zinc, or uses mechanical power, and lifts electricity to a higher level or potential.
2. Energy available is, amount of water lifted × difference of level.	2. Energy available is, amount of electricity × difference of potential.
3. If we let all the water flow away through channel to lower level without doing work, its energy is all converted into heat because of frictional resistance of pipe or channel.	3. If we let all the electricity flow through a wire from one screw of our generator to the other without doing work, all the electrical energy is converted into heat because of resistance of wire.
4. If we let water work a hoist as well as flow through channels, less water flows than before, less power is wasted in friction.	4. If we let our electricity work a machine as well as flow through wires, less flows than before, less power is wasted through the resistance of the wire.
5. However long and narrow may be the channels, water may be brought from any distance, however great, to give out almost all its original energy to a hoist. This requires a great head and small quantity of water.	5. However long and thin the wires may be, electricity may be brought from any distance, however great, to give out almost all its original energy to a machine. This requires a great difference of potentials and a small current.

You will readily understand then that for some purposes it is necessary to have our electrical energy in the shape of a small quantity of electricity falling through a great difference of potential, and for other purposes a great quantity of electricity falling through a small difference of potential. When electricity falls through a difference of potential, this difference is called an electromotive force. It would take me too long to tell you why we use two terms to express what seems to be the same thing; but briefly, the term "difference of potential" is analogous with "difference of pressure" or "head" of water, howsoever produced; whereas electromotive force is analogous with the difference of pressure before and behind a slowly moving piston of the pump employed by an unfortunate miller to produce his water supply.

The first object of my paper is to show you that electricians have very definite ideas on the subjects they are working at; that the measurements on which their work depends have exact meanings, and that there is hardly any problem in adding to man's powers which you can set before them to solve which they may not hope to do with more or less costly apparatus. Everybody knows that the civil engineer is still very far from having reached the limiting lengths or sizes to which large bridges and other structures may be built, at a greater or less cost. Everybody is competent to form a roughly correct judgment in such matters, because everybody has more or less correct notions about sizes, weight, and strength of materials. And in the same way that you may be able to guess of what the electrician may do in the future, it is necessary that you get fairly correct ideas of electrical magnitudes; and the curious fact is that, seeing how simple it is to arrive at these correct ideas, so few people possess them. On the wall-sheets II. and III., I have given such help as can be given visibly in this matter; but time will not allow of my entering into such explanatory details as I should desire.

WALL-SHEET II.—ELECTRICAL MAGNITUDES.
(SOME RATHER APPROXIMATE.)

Resistance of

One yard of copper wire, one-eighth of an inch diameter..............	0·002 ohm
One mile ordinary iron telegraph wire,	10 to 20 ohms
Some of our selenium cells	40 to 1,000,000
A good telegraph insulator	4,000,000,000,000

Electromotive force of

	Volts.
A pair of copper-iron junctions at a difference of temperature of 1° Fahr ....=	0·000,01
Contact of zinc and copper........=	0·75
One Daniell's cell .... =	1·1
Mr. Latimer Clark's standard cell..=	1·45
One of Dr. De la Rue's batteries....= 11,000	

Lightning flashes probably many millions of volts.

Current measured by us in some experiments:

Using electrometer................=	almost infinitely small currents.
	Weber.
Using delicate galvanometer.......=	0·000,000,000,040
Current received from Atlantic cable, when 25 words per minute are being sent......................=	0·000,001
Current in ordinary land telegraph lines .. .. =	0·003
Current from dynamo machine.....=	5 to 100 webers

In any circuit, *current* in webers = *electromotive force* in volts ÷ *resistance* in ohms.

WALL-SHEET III.—RATE OF PRODUCTION OF HEAT, CALCULATED IN THE SHAPE OF HORSE-POWER.

In the whole of a circuit = *current* in webers × *electromotive force* in volts ÷ 746.

In any part of circuit = *current* in webers × *difference of potential* at the two ends of the part of the circuit in question ÷ 746.

Or, = square of current in webers × resistance of the part in ohms ÷ 746.

If there are a number of generators of electricity in a circuit, whose electromotive forces in volts are—E_1, E_2, etc., and if there are also opposing electromotive forces, F_1, F_2, etc., volts, and if C is the current in webers, R the whole resistance of the circuit in ohms, P the total horse-power taken in at the generators, Q the total horse-power converted into some other form of energy and given out at the places where there are opposing electromotive forces, H the total horse-power wasted in heat, because of resistance, then:

$$C = \frac{(E_1 + E_2 + \text{etc.}) - (F_1 + F_2 + \text{etc.})}{R}$$

$$P = \frac{C}{746}(E_1 + E_2 + \text{etc.});\quad Q = \frac{C}{746}(F_1 + F_2 + \text{etc.});$$

$$H = \frac{{}^2CR}{746}$$

The lifting-power of an electromagnet of given volume is proportional to the heat generated against resistance in the wire of the magnet.

The future of many electrical appliances depends on how general is the public comprehension of the lessons taught by these wall-sheets. If a few capitalists in London would

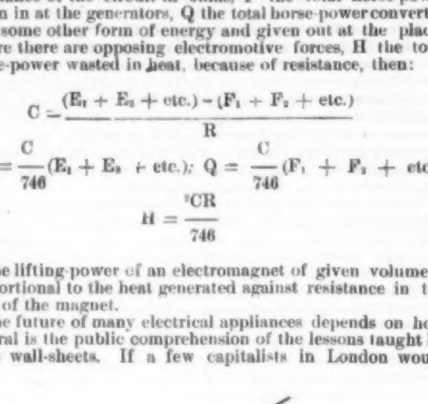


FIG. 1.

only spend a day or two in learning thoroughly what they mean, I am quite sure that electrical appliances of a very distant future would date from a few months hence.

It is not necessary for me to tell you now that electrical energy may be produced. Nor need I waste time in speaking of how it may be transmitted to a distance by means of insulated metal wires. A more important fact is that, when electricity is flowing in a wire, I can transform part of its

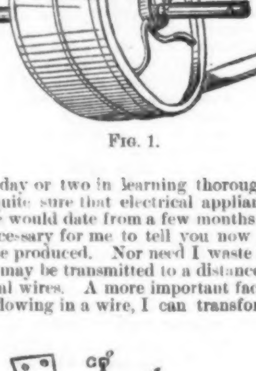


FIG. 2.

Another arrangement giving greater range.

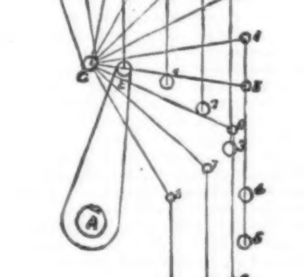


FIG. 3.

When tangential strain is small, this method is used.

energy into other shapes. For instance, here is an iron wire of 2 ohms resistance. Suppose this to be in a cold room, and I turn on the electricity tap. (An electric machine, driven outside by a gas-engine, is here my source of energy.) This wire is now getting a supply of electrical energy, and is converting it into heat. Mr. Andrews tells me that there is now a current of 20 webers flowing through the wire, and hence the wire is giving out more than one horse power in the shape of heat. Some of you may have thought that very little heat can be given out by such a wire; but these are the exact figures, and you can all see that they represent a pretty large supply. When the current has been flowing for a short time, the neighborhood of this wire will be found unpleasantly warm, and I can assure you that the use of this instrument for certain measuring purposes is very disagreeable in the summer time. It is hardly necessary to say that a wire, through which a current is flowing, may be made to give out its heat for a great variety of purposes. The temperature may be pretty much what we please. Thus, I turn the tap, and this wire gives off very intense heat. (Experiment shown.)

I had asked my friend, Mr. Andrews, to boil water for you by means of a hot spiral of wire; but he has given us something of his own which is very much better. You see that I turn this tap, and so pass this current among all these little bits of carbon; first we have bright spots of light here and there stealing from point to point; then these lights fix themselves in definite places, and round them the carbon gets red hot, until we get in two minutes the most perfect form of fire for heating a room or boiling a kettle that I have ever seen. I have in vain tried to get Mr. Andrews to exhibit before you to-night his exquisitely simple plate electric light. I have watched it burning, and know that it has a future before it, if it were only from the fact that it burns steadily for a whole week with a powerful arc light without renewal of the carbons, and yet these carbons might be put in one's pocket, and the lamp thrown about anyhow, without risk of anything getting out of order. The excessive caution of the inventor prevents my showing you this simple little lamp. My own lamp is here before you, but beyond telling you that it is very simple, and that only one magnet is employed in the regulation and separation work, I may not detain you. I now turn another tap, and the strip, through which the current passes, becomes white hot, and we call it, vaguely, an electric light. (Experiment shown.) This is the incandescent light which has been proposed for use in ordinary houses. It is, confessedly, not economical, but it is very convenient for chamber use. I now turn another tap, and you see a powerful Serrin lamp, which I mean to leave burning. You know now that we can convert electrical energy into heat and light; but the question is, how much of a result do we get for the power expended?

Professor Ayrton and his students measure at Cowper street, 1st, how much gas is being used by his gas-engine; 2d, how much horse-power is being actually given to his electric machine; 3d, how much current is produced through external circuits by his machine; 4th, the resistance of these circuits. He can now calculate exactly how much horse-power is expended in any part of these circuits; and also how much light is actually given out by an electric lamp.

I must now try to give you an idea as to how these measurements are made. The very elegant dynamometer employed by our chairman to measure the power which is being transmitted to a machine, I am not at liberty to describe. The plan devised by Professor Ayrton and myself is capable of being applied at very small cost to existing shafting in factories, so that the power given to any shaft may be known. A is a shaft which is to receive power. B is a loose pulley driven by a belt. C D is a wheel whose rim is fixed to the rim of B; its crooked arms are made of flexible steel, its boss being keyed to the shaft. Evidently B can no longer be called a loose pulley; if it turns it must cause the shaft to turn, but the turning moment is accurately represented by a certain amount of yielding of the steel arms of C D. If this yielding is known, and also the speed, the horse-power transmitted is also known. For, so far, we copy the principle of General Morin. But instead of using his elaborate system of measurement, we simply convert the tangential strain into a radial motion which is visible. This may be done in various ways, of which the following is the most simple. A stiff arm, E A, is fixed to the shaft at A; at E, and at a point C, of the wheel, the ends of two light links are pivoted, which are hinged together at G, where there is a bright bead. Evidently, if the distance, C E, becomes large, because much power is being transmitted, the bead, G, moves out from the center, and therefore the circle of light described by it has a greater radius. The arrangement shown in Fig. 3 is sometimes more convenient.

We measure the distance of the bead from the axis by means of a scale supported level with the shaft. Other dynamometers, which have till now been in use are shown in the diagrams. For measuring very strong currents, such as are used in electric lighting, Professor Ayrton and myself have devised this "dead-beat" galvanometer. Without going into a detailed description of the instrument, I may mention that it possesses the following great advantages. Not only can the strength of any current be read off at once in webers, but the user can at any moment test his own instrument, or graduate it, as it is technically called, by employing only the weak current produced by a single Daniell's cell. This result is arrived at by the device of causing the weak current to circulate 60 times round the magnets, while the strong current only goes round six times; a special form of commutating arrangement enabling the very same wires in the galvanometer to serve for both strong and weak currents; hence, comparisons can be made, not merely approximately, but with absolute accuracy, even if the wires are wound on the galvanometer quite carelessly.

The instrument shown in this diagram may be graduated in the same manner, as it is also provided with the same kind of commutator. We call it an arc-horse-power measurer, because its deflections are proportional to the product of difference of potential established in an electric light, into current flowing through the arc, or incandescent carbon, and hence these deflections (see wall-sheet II.) show at a glance the horse-power given out at that place. The electro-dynamometers of Dr. Siemens and of Mr. Andrews are here before you, and may be used during the reading of the paper to measure currents. Mr. Andrews simply uses a steel-yard to balance the attraction between two coils, when the current flows in them when they are at a fixed distance asunder, and he, therefore, like Dr. Siemens, measures the mean square of the current flowing. These instruments have the disadvantage that an ordinary pair of scales has in comparison with a spring balance, viz., that a sudden temporary change in the thing weighed cannot be measured; but they have the advantage of great accuracy in the measurement of a constant effect.

To measure the light itself in standard candles, the students in the course of electric lighting, at Cowper-street, employ our photometer (Fig. 5), of which three specimens are before you, and a drawing on the wall. The principle of old methods of measurement of strong lights was to weaken the intensity of illumination of a screen by taking the screen

far enough away. Only in this way could the illumination of the screen by the electric light be made equal to the illumination of a similar screen by a standard candle. Our plan of weakening the light from an electric lamp is not by going forty or eighty feet away from it—for people who deal with electric lamps do not often possess a large enough chamber with blackened walls—but by letting, instead, the light pass through a concave lens. The principle is then exceedingly simple. Mr. Wormell has been making a few measurements while I have been talking, and I see that this electric light seen through green glass has varied from 2,214 to 2,136 candles in the last three minutes. Sir William Thomson suggested to us to make two measurements, one through green and the other through red glass, for reasons which must be obvious. Anyone who may wish it will have an opportunity of measuring the power of an electric light for himself after the lecture.

From all this you will see that perfect methods exist for measuring the power which is being given out as heat or light in any part of a circuit, as well as the power given to the electrical machine. In fact, we have a perfect measure of what is called the efficiency of our arrangement.

It is hardly necessary to tell you that every house and every street may be lighted electrically. Into the proof that, in the future, arc lamps of thousands of candle power at elevations proportional to the square roots of their powers, will be used for large spaces, and that incandescent lamps of only hundreds of candle power are suitable only for private houses; into a consideration of these statements I

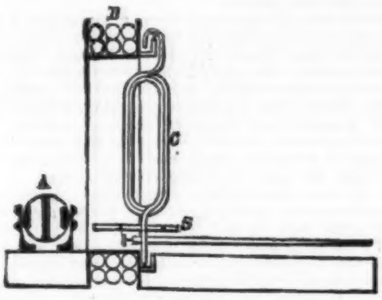


FIG. 4.

B, fixed coiled cable of ten strands, which may be used in "paralled arc" or in "series," by means of the commutator, A.
C, movable coil.
S, spiral spring.

shall not enter, because Professor Adams is dealing with the question in his Cantor lectures. You all, in one way or another, feel that electric lighting is a foregone conclusion. But, perhaps, you were not aware that buildings may be heated by electricity. The neighbors of this iron wire will say that it gives out a considerable quantity of heat, but whether the heating may be performed economically will depend on the story told us by the measurements which have been made. Now let me turn my tap again. I let my current pass through this insignificant little dynamo machine, and you observe that it is in motion; not only is it in motion itself, but it is driving this lathe. A machine is receiving mechanical energy outside, it converts this into electrical energy, which is conveyed by wires into the room and to the machine before you, where it is converted into mechanical energy again. I think I shall never forget the astonishment of a workman in Sheffield, who had put up a saw-bench for us at Professor Ayrton's lecture, and who was about to rehearse his part. He looked at the montionless saw, he had his hand on the wood, he saw there was a belt from a little mite of an electric machine, two wires dangled from the ceiling to the machine, and this was all. What notions of being played with came into his mind I do not know, but when, at the distant place, a water-engine was started to

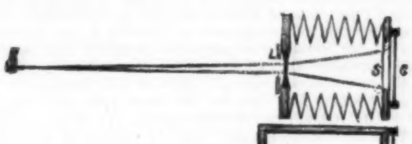


FIG. 5.—DISPERSION PHOTOMETER.

E, electric light.
C, standard candle.
S and T, screens of tissue paper.
G, H, plates of green or red glass.

$$E, \propto \frac{EL^2 . LS^2}{CT^2}$$

drive the distant machine when the saw set off nearly at its full speed, and the two dangling wires were evidently the only methods of communication, this thoughtful workman's face expressed in full perfection the absence of all his reasoning powers. I do not wish you to lose your reasoning powers, but it is necessary that you should get thoroughly impressed with the notion that the power to drive this lathe is actually being transmitted through these limp and motion less wires.

I should like to be able to hold that machine motionless, and to prove to you that the current flowing through the wires is immediately diminished when the machine begins to move. In fact, I want to show you that this machine produces an electromotive force, which is in opposition to that of the distant machine. You see that we are just able to hold it, and now I am informed that the current flowing is 19·5 webers, whereas if we let it run, and drive the lathe and the sewing-machine and this fan, you will find that the current is diminished. It is 11·2 webers, or about half what it was before. It is not necessary to give you further examples of this transmission of power by electricity, but on account of the evident importance of the matter to the health of the community, I will give you one more, and I turn the tap, and you all see that the insignificant little machine is driving a ventilator. This ventilator might be used in a chimney in the summer time when fires are not in use, or in any suitable outlet from rooms; and pray remember that mechanical ventilation is ever so much more efficient than what is called natural ventilation, in which advantage is taken of the lightness of warm gases.

Now, what do these examples show you. They show that if I have a steam-engine in my back yard, I can transmit power to various machines in my house, and if you measured the power given to these machines, you would find it to be less than half of what the engine driving the outside electrical machine gives to it. Further, when we wanted to think of the heating of buildings and the boiling of water, it was all very well to speak of the conversion of electrical energy into heat, but now we find that not only do the two electrical machines get heated and give out heat, but heat is given out by our connecting wires. We have then to consider our most important question. Electrical energy can be transmitted to a distance, and even to many thousands of miles, but can it be transformed at the distant place into mechanical or any other required from of energy, nearly equal in amount to what was supplied? Unfortunately, I must say that hitherto the practical answer made to us by existing machines is, "No;" there is always a great waste due to the heat spoken of above. But, fortunately, we have faith in the measurements of which I have already spoken, in the facts given us by Joule's experiments, and formulated in ways we can understand. And these facts tell us that in electric machines of the future, and in their connecting wires, there will be little heating, and therefore little loss. We shall, I believe, at no distant date, have great central stations, possibly situated at the bottom of coalpits, where enormous steam-engines will drive enormous electric machines. We shall have wires laid along every street, tapped into every house, as gas-pipes are at present; we shall have the quantity of electricity used in each house registered, as gas is at present, and it will be passed through little electric machines to drive machinery, to produce ventilation, to replace stoves and fires, to work apple-parers, and mangles, and barbers' brushes, among other things, as well as to give everybody an electric light.

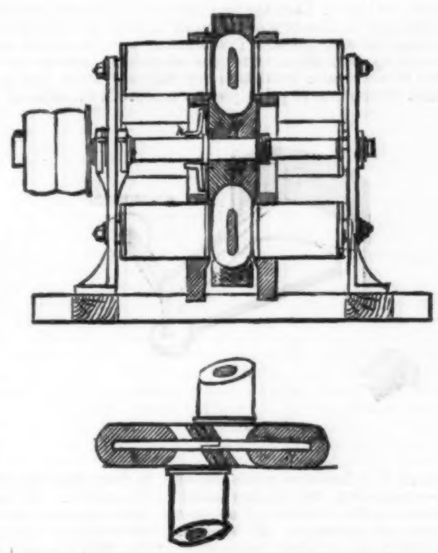


FIG. 6.—PERRY'S DYNAMO MACHINE.

Probably you think it very strange that I should show you the inefficiency of electric transmission of energy, and then make this very bold assertion. Well, the fact is, that the ordinary electrical machines in use have not been constructed with a view to economy. They have been constructed to show that brilliant lights and considerable power may be produced from small machines. They have, at a comparatively small cost, attracted attention to the fact that electricity is an important agency. In so far they have done well; but on the other hand they gave rise to the well-known assumption that 50 per cent. of the mechanical power given to the generator was the maximum amount which could be taken from the motor. The true solution of the problem of transmission of power was, I believe, first given by Professor Ayrton in his British Association lecture at Sheffield. It had been supposed that to transmit the power of Niagara Falls to New York, a copper cable of enormous thickness would be needed. Mr. Ayrton showed that the whole power might be transmitted by a fine copper wire, if it could only be sufficiently well insulated. He also showed that, instead of a limiting efficiency of 50 per cent., the one thing preventing our receiving the whole of our power was the mechanical friction which occurs in the machines. He showed, in fact, how to get rid of electrical friction. I will briefly give you our reasons. A machine at Niagara receives mechanical power, and generates electricity. Call this the generator, and remember that wall-sheet, III., teaches us that the mechanical power is proportional to the electromotive force produced in the generator, multiplied into the current which is actually allowed to flow. Let there be wires to another electric machine in New York, which will receive electricity, and give out mechanical work, as this machine does here. Now, I showed you a little while ago, that this machine, which may be called the motor, produces a back electromotive force, and the mechanical power given out is proportional to the back electromotive force, multiplied into the current. The current, which is, of course, the same at Niagara as at New York, is proportional to the difference of the two electromotive forces, and the heat wasted is proportional to the square of the current. You see then, from wall-sheet, III. that we have the simple proportion—power utilized is to power wasted, as the back electromotive force of the motor is to the difference between electromotive forces of generator and motor. This reason is very shortly and yet very exactly given in wall-sheet IV.

WALL-SHEET IV.

Let electromotive force of generator be E; of motor F. Let total resistance of circuit be R. Then if we call P the horse-power received by the generator at Niagara; Q the horse-power given out by motor at New York, that is, utilized; H the horse-power wasted as heat in machines and circuit; C the current flowing through the circuit.

$$C = \frac{E - F}{R}$$

$$P = \frac{E(E - F)}{746\ R}$$

$$Q = \frac{F(E - F)}{746\ R}$$

$$H = \frac{(E - F)^2}{746\ R}$$

$$Q : H :: F : E - F$$

To put it more shortly still, the power wasted is proportional to the square of the current flowing, whereas the power utilized is proportional to the current, and also to the elec-

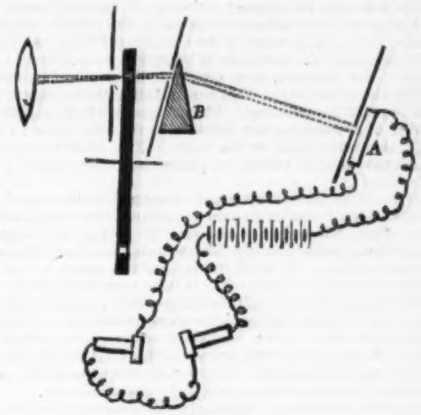


FIG. 7.

tromotive force of the motor. The greater, then, we make the electromotive forces, the less is the loss of power in the whole operation. Perhaps you will see this better from the water analogy. A small quantity of water flowing through a water-main may convey a large amount of energy, if it only has sufficient head. The frictional loss of power is independent of the head, but depends very much on the quantity of water. In the model before you is the water analogy. (Experiment shown.) A is a reservoir, kept filled with water by a steam pump, which draws the water from the sea level, K K. Water flows from reservoir, A, to distant reservoir, B, where it drives a turbine giving out work due to its head, B K. The current from A to B, through the communicating pipe, is the same always, so long as A and B are at the same difference of level, and therefore the frictional loss of energy is always the same, whereas the work utilized from B, by driving the turbine, increases proportionally to the height of B above sea level.

The result, then, to which the above laws led Professor Ayrton and myself was that for the future development of the transmission and distribution of electric energy it will be necessary to use electric machines of great electromotive force. Indeed, so important must this principle become, that we believe there is a future in this direction for the em-

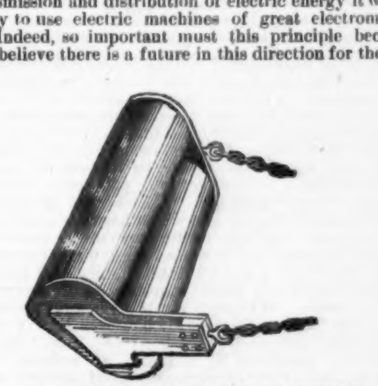


FIG. 8.—SUBMARINE COAST WARNING.

An electro-magnet, with vibrating armature, giving out loud musical note.

ployment of plate electrical machines, such as that of Holtz. Now the electromotive force of an electric machine may be increased in three ways: 1. By increased speed, as you easily see when I turn this magneto machine more rapidly. 2. By increased strength of magnetic fields. 3. By increasing the length of wire on the moving armature. Of these methods the first is most important. Now, if iron is used in the armature, since it is magnetized and demagnetized very rapidly, its coercitive force prevents this magnetization and demagnetization being as complete at the high speeds I contemplated as it is at the ordinary speeds of the present day. I say this in spite of the fact shown by some unpublished experiments of ours, which imply that the magnetization and demagnetization of a bundle of fine soft iron wires are as complete when effected sixty times per second as when effected once per second. Besides this, a very considerable quantity of heat is developed in such rapid magnetization and demagnetization as does occur. The electric machines of the future will, I am convinced, be without iron in their movable parts. High speeds necessitate careful construction and the balancing of moving parts, and great attention being given to rubbing surfaces. By rubbing surfaces, I do not merely mean the bearings of the machine, but the commutator, which is rubbed by the collecting brushes. Much of the waste of energy by mere mechanical friction which occurs in electric machines occurs at the brushes; but, hitherto, other waste has been so great that this might be neglected as unimportant. But it is very important in the machines of the future. The loss of energy by friction is proportional to the number of revolutions per minute, and to the diameter of the rubbing surface. I have given considerable thought to the reduction of this friction, and have arrived at a form of commutator shown at A in the diagram (Fig. 4), which largely diminishes the loss. The parts of the commutator must be firmly fixed, but they must also be well insulated from one another,

therefore, they must be separated by some rigid insulator, such as ebonite, at the places where they are screwed up; hence they are necessarily far apart at these places. If they are rubbed at these places, however, there will be a great loss of power in friction, and hence they ought to be bent in towards the axis of rotation, where they may be insulated from one another by narrow air spaces, and where they may be rubbed by the brushes, with only a small waste of energy. This plan I have proved to be quite feasible. In the larger machines of the future, its importance will become much more manifest than it can be in existing machines. This frictional principle is illustrated by the model before you. Here are two surfaces, making the same number of revolutions per minute. If the same amount of rubbing occurs, you observe that when I rub the surface of larger diameter, there is great loss of energy, and the motion is stopped; whereas, when I rub the surface of smaller diameter, there is only a small loss of energy, and the motion is not stopped. (Experiment shown.)

This necessity for a great velocity of moving coils past fixed magnets, necessitates increase of size of the armature, because for a given velocity the centrifugal force tending to burst the revolving armature is inversely proportional to the radius. For instance, here are two light wheels, made in exactly the same way. You can examine their construction at the end of the meeting. They are rotated at a different number of revolutions per minute, so that the actual velocities of their rims shall be the same. You observe that the rim of the smaller bursts in pieces, and the larger is unhurt.

There is another important reason for increased size, namely, that of similar dynamo machines, one twice as large as the other; the larger is capable of giving out eight or more times as much energy for the same number of revolutions per minute. It would delay me too much to go into this question of size fully; but if it be remembered that the electromotive force of each moving coil is proportional to its area, then, without taking into account increase of strength of magnetic field, which certainly occurs with larger machines, we get eight times as much effect for double the size. Electric machines of the future will then, probably, be of great size, moving with exceedingly great velocity.

The third method of increasing the electromotive force by having greater lengths of wire in the armature is always available, but inasmuch as every increase so produced causes a proportional increase in the resistance of the circuit, and therefore a waste by heating, this method is not quite so economical as the increase of speed method.

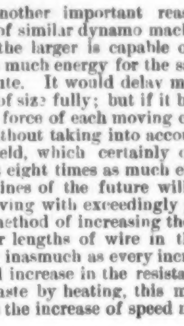


FIG. 9.

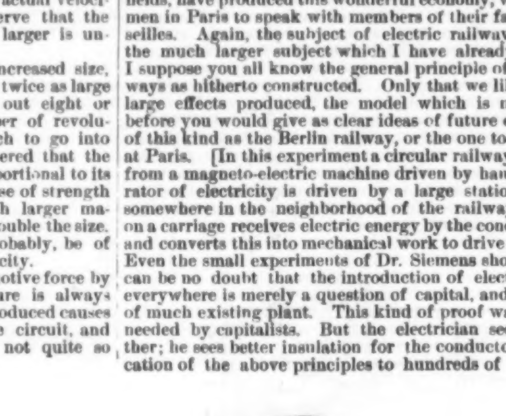


FIG. 10.

It is to be remembered that the lifting power produced in an electro-magnet of given size is simply proportional to the heat produced in the wire on the magnet, and if it is our object to diminish this heat, we must discard all idea of working the magnets of electric machines by their own currents. In fact, the function of dynamo-machines, like these I have been using, will, in the future, be to feed the magnets of larger machines, or else they will give place altogether to magneto electric machines. I have now given you, very briefly, some of the reasons which have occurred to us for believing that very large continuous current machines, with separate exciters, or, perhaps, even magneto-electric machines, driven very fast by steam engines, will have an important place in the future transmission of energy by electrical methods. With such machines it would be possible to heat, light, and ventilate all the houses in New York, and to give to large and small workshops the power required to drive their machinery by means of an ordinary telegraph wire (but with some exceptionally good method of insulation), transmitting energy from as great a distance as the Falls of Niagara.

When I speak of what will be done in the future in this direction, I can speak with perfect certainty. It is useless to tell us that existing machines are not economical. As I have already said, existing machines have been made with a very different purpose; to show that much electrical energy and striking light effects may be produced by a small and portable machine. They have drawn the attention of capitalists to electric lighting and electric railways, and in this way have done great service. Calculations of possible economy in the future, deduced from their action merely, must, however, be quite misleading. But if the facts given in this wall-sheet are correct—and, fortunately, there can be no doubt of their correctness—the practical transmission of all kinds of power to all distances, the supply of large and small quantities of light and machine power to all parts of a city like London from a single center, and a consequent return to that old state in which in many trades it was possible to dispense with the congregation of great numbers of men in large manufactories, is a thing to be looked forward to with perfect certainty. I need hardly tell you that heating houses by electricity will completely get rid of the smoke nuisance. I have been dealing with general principles, and electricians will take various plans to carry out the idea put before you. In my own machine, exhibited here, and also drawn upon this diagram (Fig. 6), I have endeavored to carry them out in my own way. This is the largest machine which I could induce my kind friends, the firm of Messrs. Clark & Muirhead, to construct for me. I would, were money enough available, apply the principle to coils wound obliquely on the thin rim of a great fly-wheel of a large steam-engine, fixing magnets obliquely to one another on both sides of the rim.

I have so much pecuniary interest in the future of this machine that it would take from the impersonal character of the lecture if I brought it before you too prominently. Its performance may be examined into at the manufactory. If time allowed, I would rather dwell on the enormous social phenomena which are preparing to develop themselves. England is a very rich country. She can afford, even through her Government, which dispenses only a small portion of her wealth, to carry out great enterprises at the ends of the earth. By her canals and roads, and then by her railways, she has made herself comfortable, and has added to her wealth. Adding to her wealth is an accidental effect, perhaps, but adding to the happiness and health of the poorest people in this cradle of the Anglo-Saxon race is certainly the most important work to be effected by the wealth of England. To do this, through the agency of electricity, will not prove a bad financial investment.

Leaving this very large subject, let me speak of a few of the applications of the above principles which have a future before them. The development of the telephone and of telephone exchanges, until every person in London can speak directly with every other Londoner, and, indeed, with every other person in the country; this, as you all know, is quite a settled matter, although, no doubt, there are little difficulties still to be surmounted. At one end of a telephone wire there is a generator, a magneto-electro machine, which receives sound energy, and gives out electricity. At the other end there is a receiver or motor, another such machine, which receives electric energy and gives out sound. We have, in fact, a simple example, and one of the most economical examples I know of, for the transmission of power by means of electricity. Quick speeds caused by vibrations of many hundred times per second, and strong magnetic fields, have produced this wonderful economy, which enables men in Paris to speak with members of their family in Marseilles. Again, the subject of electric railways is a part of the much larger subject which I have already dealt with. I suppose you all know the general principle of electric railways as hitherto constructed. Only that we like to observe large effects produced, the model which is now working before you would give as clear ideas of future constructions of this kind as the Berlin railway, or the one to be exhibited at Paris. [In this experiment a circular railway was worked from a magneto-electric machine driven by hand.] A generator of electricity is driven by a large stationary engine, somewhere in the neighborhood of the railway. A motor on a carriage receives electric energy by the conducting rails, and converts this into mechanical work to drive the carriage. Even the small experiments of Dr. Siemens show that there can be no doubt that the introduction of electric railways everywhere is merely a question of capital, and the sacrifice of much existing plant. This kind of proof was very much needed by capitalists. But the electrician sees much further; he sees better insulation for the conductor, and application of the above principles to hundreds of miles of rail instead of a thousand yards; he sees, in fact, that the larger the experiment, the greater must be its success. He looks forward to the absence of a vitiated atmosphere in our underground railways. He sees that the weight of rails (for there will be no heavy locomotive in the future—each carriage will have its own driving and braking machinery), and the cost of bridges, and wear and tear of permanent way, may become less than one quarter of what they are at present; he sees, in fact, all the advantages that will arise, when, instead of making a heavy steam-engine travel backward and forward with carriages, the carriages alone travel, and the steam-engine is not near the railway at all. In that case, also, all the energy at present wasted in stopping a train will simply be given back to the generator.

I have mentioned electric lighting and telephones and railways, because I know that many of you must have expected to hear of them, but I mainly wish you to consider these appliances as examples simply of the transmission of power by electrical means. In the same way I might refer to a countless number of other appliances, giving you a mere catalogue of them; but, from the ordinary house-bell to the complicated arrangement by which my brother regulates the weirs on a river to prevent floods; from the time-regulating luxury of certain clockmakers, to the quadruplex telegraphy of Muirhead and Winter, they are simply methods of transmitting energy by electricity, and as such, their economical development depends on the recognition of the above principles. Take, for example, the case of ordinary telegraphy. There can be no doubt that it is absurd to fill large houses with tens of thousands of voltaic cells to work telegraph lines. But it is not sufficient for the post office authorities to feel the annoyance, and merely try to replace batteries with such a machine as you see before you—a machine of but one *ohm* resistance, while every mile of telegraph wire may have twenty ohms resistance. I am sure that everybody belonging to the telegraph department will be satisfied with a change that gives them one dynamo machine for all those thousands of sloppy voltaic cells; and there is no longer any excuse for further delay, since Mr. Schwendler has been perfectly successful in working long telegraph lines in India in this very manner.

When we think of electricity as an agent by means of which energy may be transferred and altered, it is natural to ask if, by means of it, energy can be stored up. If we could obtain an efficient method of storing energy, the result would be of very great importance in a variety of ways. Thus, if all the work obtainable from the tide filling and emptying great shallow basins, could be stored up, so that it might be given out steadily, and only at our pleasure; if all the work obtainable from wind-power, which is constantly varying, could likewise be stored up, so as to be readily available, a long-standing difficulty would be got rid of, which has hitherto prevented the working out of large schemes for the utilization of these sources of natural power. And not only in these large cases, but in a countless number of other ways, is it important to possess means of storing energy. In the manufacture of gunpowder, and in many chemical operations, energy is stored up; but no such method can ever become economical. It has to be remembered, however, that electrical operations may be made as economical as we please; and, however, insignificant the method may appear to be just now, it may assume great importance in the future, from the fact that, with the exception of the lifting of heavy bodies to higher levels, an electrical method of storage may be made more economical than any other. Now when I charge this Leyden jar (experiment shown) you know that I store electrical energy, and I can use my stored energy at any future time if the insulation of my jar is good. Thus I have converted a small store into heat and light. (Experiment shown.) Again, I can use this store at any time to give itself out at a distant place. This is a very small store. But now observe that my thermopile has been working for nearly an hour, and some time ago it had filled these two test tubes with oxygen and hydrogen. With these two gases I can produce, as you all know, a most intense heat. You all know that this lime light is produced simply through my having such a store in these iron bottles which you see before you. Remember that these gases might be kept stored up for as long as we like, and that if a windmill worked a magneto-electric machine it could produce such a store working now fast, now slow. Well, but I can take this store and convert it again into electricity with very little loss. You will see that it can produce an electric current if we have two similar metal plates in the positions you see them in, and if I connect these metal plates through the galvanometer (experiment shown), you have there evidence of a current, this deflection of the needle of the galvanometer. This current will continue to flow, and the electric energy will continue to be given out until all the store of gas disappears.

Instead of using that weak thermopile, suppose I had used this strong current produced by the outside engine, you see how much more rapidly my store is formed. (Experiment shown, in which the gases formed were used to produce an oxyhydrogen lime light.) I grant that the elaboration of this gas battery into a compact generator sufficiently powerful to produce very large effects, is a problem of some expense for future workers; but give it to any electrician, and make it worth his while, and I believe that such a generator might be constructed in a very short time.

To introduce the next part of my subject, let me ask: Can anybody hear the sound made by a puff of air as it passes through the hole in this cardboard disk? (Experiment made.) Nobody heard it, or the difference produced when the air was stopped by the cardboard. But suppose I repeat this operation several hundred times per second, you can all hear the powerful musical note given out. (Experiment.) You see, then, that the rapid recurrence of effects may be very sensible to us, although one such effect may not be sensible. In the same way, if light streamed through one of the holes in this brass disk into your eyes, it would not produce a very striking effect; whereas Professor Tyndall says that when such a disk as this was rotating so as to let the light falling on his eyes be very rapidly intermittent, he experienced the most extraordinary sensations. Again, if I very much alter the magnetic field in this telephone, by bringing a powerful magnet near it, with great care in listening I hear the faintest sigh, due to the diaphragm settling itself into a new position, its vibrations dying away as it does so; and if I brought a small magnet near, I should hear nothing. And yet the change of magnetism which produces the loud telephonic effects which we listen to is almost infinitely smaller. Why is this? It is due to the rapid recurrence of the effects. Now you are all aware of the importance of the telephone as a method of communication; I believe that a much greater importance is in store for it as a laboratory appliance.

Here is a selenium cell through which I can pass a current of electricity from this large battery, which also passes through these two telephones, which I can hold to my ears. When light falls on this selenium its electric resistance is diminished, and a stronger current passes. This is a property discovered by Mr. Willoughby Smith. Now I cannot hear in the telephone any effect produced by letting light suddenly fall on the selenium. The difference of current produced in the very case before you is only one two-thousandth of a weber. But if I rotate this brass disk so as to make the light fall with intermittence several hundreds of times in a second on the selenium, I can distinctly hear a musical sound. This is what Professor Bell has been exhibiting lately, and it constitutes the principle of the photophone.

Now, to give you an idea of the new ground which the use of the principle of recurrence is opening up in laboratory work, let me speak of an experiment which is now in progress. Professor Bell spoke in his lecture of having tried to stop the intermittent rays of light of this instrument by a sheet of ebonite like this, but he found that there was still a very faint sound from the telephones. Well, it occurred to Professor Ayrton and myself that if ebonite is transparent to some kind of invisible radiation, then in all probability it is capable of refracting such invisible rays. So we obtained this ebonite lens, and two prisms, and tried. We thought the lens would bring the invisible rays to a focus, but as our lens was not mounted, so that we could move it parallel to itself, and as the rays are, of course, quite invisible, so that our eyes cannot help us to focus the ebonite lens, we did not succeed in this very delicate experiment, which the following experiment, however, shows must ultimately be successful. Next, we placed the cell at A, in this diagram (Fig. 7), and found that it gave out no sound, being beyond the range of the beam of intermittent light. We placed the prism in the position, B, in which you see it, and, to our great satisfaction, a sound was heard. You must remember that this sound, and any sound obtained from light that had passed through ebonite, was exceedingly feeble. The person who listened was in another room, so as not to be in any way influenced by what he saw, and his preciseness in detecting sound was determined by another experimenter putting his hand in the beam of light and taking it away again. So that there could be no doubt as to the origin of the faint sounds heard. Well, the prism caused the light to bend round, and now the question was as to how much bending it produced. We provided two pieces of zinc plate, with slits cut in them. You all understand, I hope, that the most advanced physicists regard a metal as a perfectly opaque body, even to invisible rays, so that rays can only pass through the slit in our zinc. Well, we placed the slit in the zinc within a short distance of the edge of the prism, and found a position in which the rays, passing through the slit, still reached the selenium. The sound was now very faint. Then we searched on the selenium with the edge of our second piece of zinc, to find what region of the cell might be covered without destroying the sound. We found that region, and placed our second slit there. Rays passing through the first slit were now passing through the second slit.

If either was changed in position, the sound died away instantaneously. Thus, there could be no doubt of the fact that ebonite refracted that invisible beam, about which nothing else is as yet known. If our slits had been very

narrow we could have measured accurately the index of refraction, but with narrow slits the sounds were too faint to be heard in the center of London, so all that we can say at present is, that ebonite certainly refracts light, and its index of refraction is, speaking quite roughly, 1·7. Now, it is somewhat curious that this was the rough measurement which we made. For Clerk Maxwell's theory that light is propagated through space like an electro-magnetic disturbance, requires the square of the index of refraction, for light of very low refrangibility, to be equal to the electric specific inductive capacity of the substance, and it has long been known that this electric constant for ebonite varies from 2·2 to 3·5 in different specimens. The square of 1·7 is 2·89. Thus you see that this curious following out of our first idea has led to a further backing up of Clerk Maxwell's electro-magnetic theory of light. This, and other investigations which we are now proceeding with, illustrate two important things, namely, the principle of recurrent effects in the use of the telephone, has opened up a new path into unexplored nature; and secondly, the laboratory worker sees before him a hundred interesting phenomena, which ought to be investigated at once, and which he cannot take up unless he gets more apparatus, more money, and more observing eyes and working hands.

About two years ago, it struck Prof. Ayrton and myself, when thinking how very faint musical sounds are heard distinctly from the telephone, in spite of loud noises in the neighborhood, that there was an application of this principle of recurrent effects of far more practical importance than any other, namely, in the use of musical notes for coast warnings in thick weather.* You will say that fog bells and horns are an old story, and that they have not been particularly successful, but our scheme was of a somewhat different kind. In northern Japan, where fogs are the rule and not the exception, which they are in England, and where changing currents of more than six knots are common off many dangerous parts of the coasts, shipmasters are very much in the habit of using their steam-whistles, listening to the echo from the steep coasts, and judging from the interval of elapsed time what is their distance from the coast, and what is their position. But they find that on many foggy days they can, and on other foggy days they cannot use this method, because they may hear no echo, although quite near the coast. Now, it seems to be forgotten by everybody that there is a medium of communication with a distant ship, namely, the water, which is not at all influenced by changes in the weather. At some twenty or thirty feet below the surface there is an almost perfect calm, although there may be large waves at the surface. Suppose a large water-siren like this (experiment shown) is working at as great a depth as is available, off a dangerous coast, the sound it gives out is transmitted so as to be heard at exceedingly great distances by an ear pressed against a strip of wood or metal dipping into the water. If the strip is connected with a much larger wooden or metallic surface in the water the sound is heard much more distinctly. Now, the sides of a ship form a very large collecting surface, and at the distance of several miles from such a water-siren as might be constructed, we feel quite sure that, above the noise of engines and flapping sails, above the far more troublesome noise of waves striking the ship's side, the musical note of the distant siren would be heard, giving warning of a dangerous neighborhood. I have no time now to tell you of the small experiments we have made in this direction. This electric bell sounds only very faintly when in water, and yet we have been able to hear it at the distance of sixty feet along a trough of water in a place filled with the noise of much heavy machinery. We took this water-siren to Hastings for a trial in ordinary boats, but the weather was too rough at the time for boats to go out, and therefore the experiment had to be postponed. We have constructed the arrangement shown full size in this diagram, in which currents of electricity are sent from a distance sufficiently rapidly intermittent through this electro-magnet to give the natural period of vibration to this armature when in water (Fig. 8). Whether this will prove successful or not we do not know, but we feel sure that the idea is to be carried out electrically, the source of sound being a motor worked by a generator on the nearest coast. In considering this problem you must remember that Messrs. Colladon and Sturm heard distinctly the sound of a bell struck under water at the distance of nearly nine miles, the sound being communicated by the water of Lake Geneva.

Another application of the principle of recurrent effects, which may, indeed, be regarded as the earliest of such applications is this multiple telegraph of Mr. Elisha Gray, which my friend Mr. Graham has been kind enough to put in working order, so that it may be worked from this table to the telephones hanging against that wall. About this telegraph which allows a great number of messages to be sent through an ordinary telegraph wire at the same time, Sir William Thomson wrote to me in terms of high eulogium when he first examined it at Philadelphia. At present I believe that the quadruplex system is more favorably looked upon, because it has succeeded better in practice, but I am inclined to think that in the distant future it may possibly have enormous development.

In this paper I wish I could bring in as illustrations of the few great principles which are really the important factors in the future development of electrical appliances, the microphone and all the instruments which have been derived from it, but even to refer to them would take far too much time. I would end by speaking of two appliances which are of quite a different species, namely, Mr. Edward Bright's method of de-electrifying woolen yarn, and of a contrivance for seeing by electricity. In the manufacture, the woolen yarn becomes electrified by friction, and has hitherto lost its electricity very slowly, requiring to be stored for many months in damp cellars before it got rid of its electricity. Until lately nobody seems to have suspected that it was electricity which caused the fibers to stick out on all sides of the yarn instead of staying in an interlaced condition. It was found to occur most in dry weather, and was vaguely put down by Englishmen to "the weather." So very annoying was this in a dry climate, that although Bradford men and Bradford machines were taken to America, only two months in the year could really be devoted to the manufacture. Now, we have here some wool staple in the air which is being electrified by this plate machine. You see how the fibers repel one another and remain in this state. You observe, however, that these other fibers we try vainly to electrify, because they are in a partial vacuum, and electricity escapes from them as rapidly as it is formed. I will allow air to enter this air-pump receiver, and now, when the machine is worked, you see—(experiment shown)—that these fibers retain their electricity. The principle that a partial vacuum is very conducive, has long been known to electricians, but the remarkable saving in woolen manufacture, effected by applying a knowledge of the principle, was left for Mr. Bright. Mr. Bright's plan of operations is to have chambers where partial vacua may be produced. He wheels large trucks of electrical bobbins of yarn into these chambers, and takes them out very soon, unelectrified, thus performing in a few minutes an operation that used to be badly performed, in a costly manner, in half a year. Can we doubt that, when boys obtain, in all elementary schools, a little knowledge of electricity, there will be rapid additions to the number of electrical appliances?

* Since the reading of this paper my attention has been drawn to a letter in the *Engineer* of Jan. 28, 1876, from Mr. H. T. Humphreys, who there suggests the use of submarine sirens as coast warnings. Since the idea struck Mr. Ayrton and myself, we have been wondering how it escaped attention so long. We now wonder why the lighthouse authorities have made no efforts in the last five years to carry Mr. Humphreys' idea into effect.

And now let me come to the last of the developments of electrical appliances, still perhaps somewhat in the future. A picture in *Punch* of an aged couple at home seeing on their drawing-room wall an image of their children playing lawn-tennis out in India, and their conversing with some of them by telephone, first led Mr. Ayrton and myself to think of this matter. We showed that it was feasible, in a letter to *Nature* and in the *Times* about a year ago. The feasibility of the method described by us was doubted, and we therefore proved it at a meeting of the Physical Society four weeks ago. I mean to put it before you in a slightly different form. Suppose that place is York, and this is London. I have a little selenium cell at York on a certain part of this picture, and at London I can throw at a corresponding place on this screen a square of light; and suppose that the illumination of this square is governed by a little movable shutter which is attached to the needle of a galvanometer. Now when light falls on the selenium at York, an immediate change occurs in it, so that more current flows to London, and this opens the shutter. The London square is then bright, when the York selenium is in bright illumination. When the York selenium is in shade or darkness, you see that the London square is in corresponding shade or darkness. (Experiment shown.) Now suppose that we form an image of this girl with her skipping rope at York, and cause a selenium cell at York to travel across her image, and suppose that this mirror at London moves so as to cause the illumination which passes the shutter to traverse this London screen isochronously—an operation performed in several telegraph instruments. Then whenever this cell reaches a dark or shady or bright place in the image at York, there will be darkness or shade or brightness at the corresponding place in London. And now, suppose that this motion is effected rapidly enough, you are all aware that if the shutter is only quick enough in its answering motions, the image of the part of the screen at York traversed by the cell will be faithfully reproduced, and will remain on the retina at London as a distinct picture in black, and gray, and white, just like a photograph. With then, perhaps, forty such cells as this all moving in the way spoken of, or a smaller number rotating on a radial arm, it would actually be possible to show at London, not merely an image of a girl at York, but an image of a girl skipping. You will, perhaps, understand better this principle from the model. Here is a path of black and white spaces at York, over which this selenium cell be made to travel. We have continued the images to the paper above, simply to let you know when the cell is in the image of a dark place, and when it is in the image of a bright place, so that you may be able to say whether there is a faithful reproduction at London. These two frames are really tied together by this long string to make them move isochronously. In practice, I need hardly say that this function will be performed in another but quite as feasible a manner.

The cell at York is in a black part of the picture; you observe no light on that part of the screen in London. The cell at York is in a bright part of the picture; the corresponding part of the screen at London is bright. And so we find that, as the cell goes successively through dark and bright places, so the corresponding parts of the screen at London are made dark and bright. (Experiment shown.) Our shutter is not yet sufficiently dead-beat for us to make this motion rapidly.*

I had hoped to be able to show you to-night the development of this method, by using what we have called the Japanese-mirror principle. We have shown† that the most minute effects on the backs of metal mirrors, effects quite invisible when examining the polished surface of the mirror, are very visible in the reflection of a divergent beam of light. Such effects, we believe, we can produce by electro-magnets arranged radially behind a circular mirror and rotating with it. This radial arrangement of magnets will move synchronously with a radial arrangement of corresponding cells. The principle, however, is exactly the same as that shown by this model, only we know that the change of curvature at a point in a mirror will obey changes of magnetic effects more rapidly than this shutter does.

I had hoped to be able to present to you the scheme which Mr. Shelford Bidwell has proved to be feasible, of reproducing in shaded lines on paper, by electro-chemical decomposition, a picture of a distant stationary object. I understand, however, that Mr. Bidwell has been asked to read a paper here, when he will exhibit the model he has made.

In my paper read here a year ago it was the importance of giving artisans facilities for obtaining practically exact knowledge in science that I especially laid stress on. To-night I have desired, first, to show what benefits our country would receive from an exact knowledge of electrical magnitudes, and of the fundamental laws of electricity being more widely disseminated, and, second, how the principle of recurrent effects may be employed to assist our senses.

DISCUSSION.

The Chairman said, in the usual course of things he should have liked to invite discussion on some of the many points referred to in this most interesting paper, but it was now so late, that although it would be permissible for any one to make an inquiry, it would be impossible to have a full discussion. He, himself, should like to ask a question on one point, which was this: Did he rightly understand Professor Perry that the whole power of the Falls of Niagara might, under some circumstances, be transmitted to New York through a single telegraph wire? It appeared to him that the enormous difference of potential at the two ends of the wire, and the large quantity of electricity passing, would cause more than sufficient heat to fuse the wire.

Professor Perry said his notion was what had been stated, and that he believed was the logical mathematical deduction from the fundamental laws of electricity; and in this Professor Ayrton agreed with him. It was the current of electricity passing through the wire which produced the heat, and if only a very small quantity passed through it could not be fused. Now the quantity of electricity was only one factor, the energy transmitted was equal to the potential × quantity. Suppose the one were 1,000,000, and the other 1,000,000 × 1 = 1,000,000; if the one became ½ and the other 2,000,000, the product was equally 1,000,000; and if one factor became $\frac{1}{1000000}$, and the other 1,000,000,000,000, the result was still 1,000,000; the product was always the same, and the smaller factor might be made as small as you pleased. That was their contention, and though it was very probable they might never be able to produce practically the perfect insulation required, he believed the theoretical deduction was sound.

Professor Ayrton said the lecturer referred to the difference of potential, not at the two ends of the wire which transmitted the power, because, of course, where there was much difference of potential at the two ends of the wire, there would be a tolerably large current passing, but the difference of potential between the wire at each end and the earth. The difference of potential between the two ends of the wire would be exceedingly small, and, therefore, the current flowing through would be exceedingly small; but the difference of potential between any parts of the wire and the earth would be extremely large, so that much work could be put in at the generating place, viz., Niagara, and much work would come out at the motor place, viz., New York, but the current flowing through the wire, which depended upon the difference of potential between the two ends, would be exceedingly small, because, though each would be very high, they would be nearly equal to one another. Consequently the waste of energy in electric friction would be small.

The Chairman said he thought this explanation had thrown considerable light on the point referred to, which was one of great practical importance. He begged to move a hearty vote of thanks to Professor Perry for his very interesting and instructive lecture, and for the experiments which had accompanied it.

The vote of thanks was carried unanimously, and the proceedings terminated.—*Journal of the Society of Arts.*

* After the reading of the paper, it was found that even with the shutter it was nearly possible to make the motion fast enough for the retention by the retina of a complete image of the path traversed.

† See *Proceedings of Royal Society*, No. 191, p. 127, 1878.

BOSTON ARTESIAN WELL.

An artesian well is now in course of sinking on Providence street in Boston. The drilling was begun on March 1, 1880. In eleven months 1,832 feet had been penetrated. The strata bored through are reported to have been as follows:

6 feet gravel filling.	7 feet gravel,
40 feet black mud,	20 feet clay,
90 feet stiff drab clay,	137 feet slate rock.

At this depth, 300 feet, a stream of water was struck.

700 feet more of slate.
100 feet sand rock to another stream.
200 feet sand rock to another stream.

At 1,600 feet a bed of water-worn gravel was met, and the water rose to within about six feet of the surface, but fluctuated, sometimes falling to 40 feet. At 1,800 feet the water rose from 3 to 20 feet of the surface. A 6-feet stratum of limestone was next encountered, underlying which was granite, through which the drilling was proceeding at the last accounts.

LAKE TANGANYIKA.

MR. E. C. HORE writes from Ujiji to the Royal Geographical Society* regarding the still unexplained phenomenon of the long-continued rise of the waters of this lake, and the reopening of the Lukuga outlet, which he was the first to witness two years ago, that the reports at Ujiji "go to show that when Cameron was here a marked rising of the lake waters had already been observed, and that it continued from that time up to about two years ago, when the surface was eight feet higher than in Cameron's time. From that date (*i. e.* two years ago) I have observed that the waters are gradually retiring, and this at a very regular rate, except during the rains (when, however, there is no rise). Three months ago the Arabs agreed in telling me, '*Now* the lake is the same as when Cameron was here.' The partly submerged palm tree on which I had fixed a water-gauge was then just left dry, and the Arabs told me that Cameron used this tree as a target and that it was then just at the water's edge. Now all the observations and the reports I hear lead me to believe that the lake has been gradually rising for years, and that it rose until it burst open the Lukuga obstruction, first oozing through in small quantities as when seen by Cameron. That the waters should now rush through the Lukuga instead of gently overflowing is probably due to the first burst having eroded a deeper channel; for, according to the geological nature of the Lukuga gap, so will the waters cut a deeper and deeper chasm, or eventually find a permanent level and gently flow over a rocky sill. I cannot think that there could have been, just before the late bursting of the Lukuga, any more than a mere trickle of water through the obstruction there, and that of periodical occurrence and of but small amount as a drainage of the lake. But what is still unaccounted for is this: before the time of Cameron's visit this periodical rising must have been infinitesimal, if any, compared with that of the few years immediately preceding the bursting of the Lukuga, or we must do away with the ancient character of the lake. I am convinced that the lake never (or, at any rate, for very many years), was at such a height as it was two years ago. This is quite apart from any geological evidence of a different state of things in remote ages. And I cannot believe that the lake has always been rising at this rate. Now, how is it that this enormous quantity of water could rise so quickly in spite of that evaporation which has (as is supposed) been sufficient for ages to maintain it almost at a level? A succession of extraordinary rainy seasons, of which we have no evidence, would not account for it. I can bear testimony to an enormous *evaporation*, but how is it that the waters suddenly gained upon the evaporation as they had never done before?"

Mr. Hore seems disposed to connect the changes of water-level with earthquake movements. Earthquakes were occurring at the time of writing (September 13, 1880). One of the Arabs stated that some years previous an extraordinary disturbance of the lake waters occurred; a long line of broken water being seen, like a reef, bubbling and reeking with steam. The next morning all was tranquil, but the shore was strewn with masses of a substance resembling bitumen, specimens of which Mr. Hore had secured to bring with him to England. An excellent map of the southern end of the lake has been made by Mr. Hore. Latitude by stars N. and S. was observed at twelve different places, and the coast line between them laid down by compass bearings.

* Proceedings R. G. S., January, 1881, p. 41.

THE THEORY OF THE PHOTOPHONE.

MR. W. H. PREECE lately presented a paper to the Royal Society having the title, "On the Conversion of Radiant Energy into Sonorous Vibrations." The experiments described in this paper had for their object the determination of the cause of the photophonic phenomena as discovered by Professor Bell and Mr. Tainter. It will be remembered that at first Professor Bell ascribed these phenomena to the effect of intermittent light vibrations. It was not long before several scientific men in France expressed doubt as to the part played by light in the results obtained, and Professor Tyndall, in England, made several experiments in the presence of Professor Bell, which seemed to point to heat as the acting agency.

Professor Bell described the phenomena obtained, treating the cause as a matter for investigation. Mr. Preece determined once for all to investigate the cause, and he claims to have settled the question, and to show that radiant heat and not light is to be credited with causation. In the paper presented to the Royal Society he pointed out that Prof. Bell and Mr. Tainter have partially answered the question by showing that the disturbances are not necessarily due to light, for they found that sheets of hard rubber or ebonite do not entirely cut off the sounds, but allow certain rays to pass and the effect to be obtained.

M. Mercadin has shown that the effects are confined to the red and ultra-red rays of the spectrum.

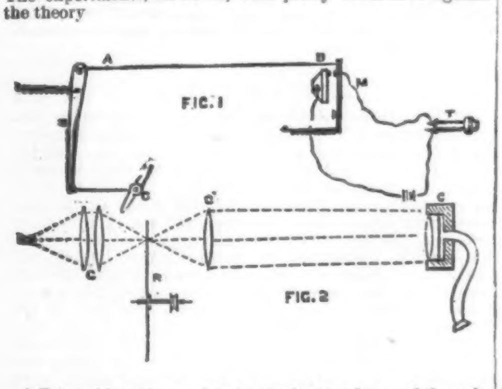
Prof. Tyndall has shown that the sound effects are a function of all gases and vapors, absorbing radiant heat, and that the intensity of the sounds is a measure of this absorption. The first series of experiments by Mr. Preece was to show that ebonite is diathermanous. The following is the result, and the numbers indicate the relative diathermancy of the substances used to the source of light used:

EXPERIMENTS IN DIATHERMANCY.

Material.		Standard candle.	Lime light.
Air		100	100
Ebonite No.	1	60	91
"	2	24·3	79·3
"	3	24·3	79·3
"	4	24·3	68·2
"	5	24·3	68·2
"	6	0	9
India-rubber	(native)	44·3	61·4
"	(prepared)	60	54
"	(vulcanized)	0	0
"	(and ozokerit)	0	0

etc.

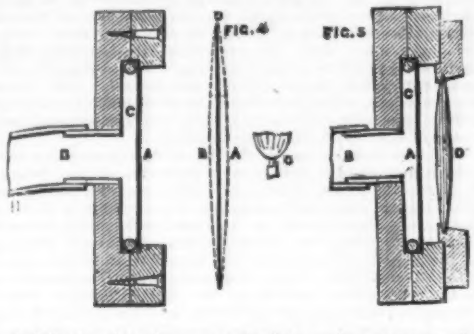
Ebonite, however, proved to be very variable, some pieces being diathermanous, while others were athermanous. This being so, shows, however, that luminosity cannot be the cause sought for, which is thermic rather than luminous. The questions raised are: Is this thermic action expansion and contraction of the mass due to the absorption of heat, or is it a disturbance of molecular pressure, or is the effect due to some other cause? Experiments were made to test whether the sonorous effects of hard disks could not be explained by the change of volume due to the impact of the heat rays. The experiments, however, were pretty conclusive against the theory



A B is a thin strip or wire six centimeters long, of the substance to be examined, fixed to a platinum "make and break," M, and adjusted to a lever, S, round whose axis is fastened a silk thread, the end of which is attached to the strip or wire as A, and whose position could be adjusted by a screw, C. Any variations due to expansion and contraction of the wire would produce intermission in the electric currents passing through the telephone, T, which, if periodically produced, would result in sonorous vibrations in the telephone. Heat from various sources and from various distances was allowed to fall intermittently on A B, but, as we have said, the results showed that the investigation was not so soon determined.

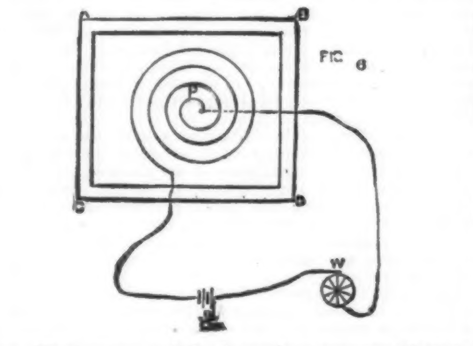
The next question to settle was whether the effect was due to a disturbance of molecular pressure, which may for short be called radiometer action. An apparatus was constructed similar to that described by Messrs. Bell and Tainter. The source of light, L, was an oxyhydrogen lime light. The rotating disk, R, was of zinc perforated with holes, which could be noiselessly rotated so as to obtain 1,000 intermittances per second. Glass lenses, G, were employed to focus the light upon the perforations of the rotating disk, and another, G¹, to render the rays parallel on the other side of the disk. A mahogany case or cup, C, to retain the disks to be experimented upon was constructed as shown in section in Fig. 3, and fixed 400 centimeters from the source, L; *a* being the disk, five centimeters in diameter, clamped on by screws; a brass tube, *b*, to which the India-rubber hearing tube, *h*, was fixed; *c*, a circular air-space behind the disk, six centimeters in diameter, and three to five millimeters deep. Cavities of various dimensions and forms, spherical, conical, and trumpet-shaped, were tried, but the ones described were those which gave the best effects.

Experiments were made with various disks of ebonite, zinc, mica, etc., blackened clean and bright, but the results were inconclusive. The effects produced by the zinc disk, though very weak, favored the theory; those given by the mica disk completely refuted it; while those given by the ebonite disks were almost of a neutral character. If D be the disk—Fig. 4—and C the source of light, then if the excursions of the disks to and fro were due to expansion from the absorption of heat, it would first bulge toward A, since the side toward the source of light would expand first. If, on the other hand, it were due to the radiometer effect, it would first bulge toward B. An extremely delicate electrical contact arrangement was constructed to determine this by means of a telephone, which recorded the excursions to and fro of the disk; but the result was sometimes in one direction and sometimes in the other. Moreover, the effect was slow, and no more than five distinct vibrations per second were obtained. This result raised the question whether in Bell and Tainter's experiments the disks vibrated at all.



A delicate microphone was fixed in various ways on the case, Fig. 3. Although the sounds emitted in the hearing tube were as intense as indicated in experiment 1, scarcely any perceptible effect was detected on the microphone. Had the disk sensibly vibrated, its vibrations must have been taken up by the case. A microphone never fails to take up and magnify the minutest mechanical disturbances. It was thus evident that the disk did not play a prime part in this phenomenon, but it appeared, as Prof. Hughes suggested, that the result might be due wholly to an expansion and contraction of the air contained in the air space, *c*, Fig. 3.

To obtain considerable effects it was found necessary to have a lens, *d*, Fig. 5, placed close in front of the disk, *a*. If the lens, *d*, was removed, and the disk left supported without any air cavity, either behind or in front of it, no perceptible sound was obtained, proving that the effects were really due to the vibrations of the confined air, and not to those of the disk. It was, therefore, determined to dispense with the disk altogether, which was done, and better results obtained. A clean case, similar to Fig. 5, was found to give no effect, but when its interior was blackened by camphor smoke it gave strong sounds. It was thus evident that the sonorous effects were materially assisted by coating the sides of the containing vessel with a highly absorbent substance, such as the carbon deposited by burning camphor. It remained to be seen how far the lens played a part in this phenomenon. Experiments show that the sonorous vibrations are due to the motions of the contained air and are independent of the disk, and that their production is materially assisted by lining the surface of the containing space with an absorbent substance, that they are dependent on the heat rays, and are not obtained where the heat rays are stopped by an athermanous diaphragm. A long series of experiments fully described by Mr. Preece in his paper shows that transparent bodies behave in an opposite way to opaque bodies. Glass and mica can be rendered athermanous and silent by a thick coat of carbon.



Zinc, copper, and ebonite produce sonorous effects by a proper disposition of carbon. The effect in the latter case might be due to a radiometer effect, though feeble in intensity, or to conduction through the mass of the diaphragm. Tests were made to determine this, the results establishing the inference that the effect is one of conduction.

Since these sonorous effects are due to the expansion of absorbent gases under the influence of heat, and since wires are heated by the transference of electric currents through them, it seemed possible that, if we inclosed a spiral of fine platinum wire, P (Fig. 6), in a dark cavity, *a b c d*, well blacked on the inside, and sent through it, by means of the wheel brake, W, rapid intermittent currents of electricity from the battery, B, heat would be radiated, the air would expand, and sounds would result. This was done, and the sounds produced were excellent—in fact, with four bichromate cells, sounds more intense than any previously observed were obtained.

Furthermore, it was evident that if the wheel brake, W, were replaced by a good microphone transmitter, articulate speech should be heard in the case of *a b c d*. This was done, and an excellent telephone receiver was the consequence, by means of which speech was perfectly reproduced. The explanation of these remarkable phenomena is now abundantly clear. It is purely an effect of radiant heat, and it is essentially one due to the changes of volume in vapors and gases produced by the degradation and absorption of this heat in a confined space. The disks in Bell and Tainter's experiments must be diathermanous, and the better their character in this respect the greater the effect; remove them, and the effect is greater still. Messrs. Bell and Tainter obtained their *timbre* and pitch notwithstanding variation in the substance of the disk, and M. Mercadier found that a split or cracked plate acted as well as when it was whole.

These facts are consistent with the expansion of the contained air, but not with any mechanical disturbance of the disks. Moreover, M. Mercadier showed that the effect was improved by lampblack, but he applied it in the wrong place. The disks may, and, perhaps, do under certain conditions, vibrate; but this vibration is feeble and quite a secondary action. The sides of the containing vessel must possess the power to reduce the incident rays to thermometric heat and impart it to the vapor they confine, and the more their power in this respect, as when blackened by carbon, the greater the effect. The back of the disk may alone act in this respect. Cigars, chips of wood, smoke, or any absorbent surfaces placed inside a closed transparent vessel will, by first absorbing and then radiating heat rays to the confined gas, emit sonorous vibrations. The heat is dissipated in the energy of sonorous vibrations. In all cases time enters as an element, and the maximum effect depends on the diathermancy of the exposed side of the cavity, on its dimensions and surfaces, and on the absorbent character of the contained gas. The remarkable property which deposited carbon possesses of reducing radiant energy to thermometric heat is strikingly shown by these experiments, and it suggests an important field for inquiry for those who are working in the region of radiant heat.

HOW IS PETROLEUM TO BE EXAMINED?

By F. SKALWEIT.

THE various results obtained these last two years by several chemists who have examined the same petroleum with a view to ascertain how far it is fit or unfit for a given purpose, induced me to look for other means than those used up to the present day, in order to discover the degree of its inflammability and its defective qualities.

The first thing to be done was to exactly determine its specific gravity and its boiling-point. Thus it was found that the various kinds of petroleum which, at a lower point of temperature, develop inflammable gases, have a specific gravity under 0·800 and even 0·789, whereas good sorts are considerably heavier, and rise to 0·824. The boiling-points, also, are considerably lower; and it is clear that the inflammability of such petroleum depends, to some extent, on its boiling-point, as follows:

				Igniting-point.
First change from	carbd.	hydrogen at	100° C.	22°
"	"	"	108	24
"	"	"	121	26
"	"	"	126	25·5
"	"	"	130	30
"	"	"	133	31
"	"	"	138	35
"	"	"	141	37
"	"	"	180	65

The latter petroleum is sold here in Hanover under the name of "safety oil" (*Sicherheits öl*), and is obtained from the factory of Dr. W. H. Lepenau, of Salzbergen. According to these experiments, petroleum, which in the usual process of distillation develops inflammable hydrogen gases, at 140° or lower, will probably not answer the requirements of the English law, which prescribes that petroleum shall emit inflammable gases at 38° C. only. This has, up to now, been the case with the various kinds of petroleum which have come within my observation. Many more experiments will be necessary to be able to legally determine the lowest permissible boiling-point instead of the igniting-point. Should this become feasible, the matter would be greatly simplified, to the general satisfaction of most chemists. Whatever may be said to the contrary, all the apparatus which have been used up to the present day to fix the igniting temperature require that the experimentalist shall work his apparatus for some days at least before he can obtain concordant results; they presuppose constant practice and proper management.

I hoped, however, to find a simpler way than that of determining the specific gravity and boiling-point in fixing the angle of refraction; and as I have, for several years already, consulted Abbé's refractometer in examining crude glycerin and oil, I made use of the same for analyzing petroleum. The experiments were made at a temperature of 17° to 18° C. The index of the alidade was so adjusted that water of 17° to 18° C. showed an average of $n = 1·3330$ from both results.

Specific Gravity.		Distillation.	Index of Refraction.	Igniting point.
0·7942	to	130°—1%	1·4431	29° C.
0·8242		180 —0%	1·5549	65
0·7920		110 —2%	1·4265	24
0·7930		not quoted.	1·4289	28
0·7937		132°—1%	1·4320	32
0·8012		141 —1%	1·4489	38

These few data, which might be considerably increased, show at a glance a great difference in the index of refraction with various kinds of petroleum; but its irregularity in rising and falling, compared with the igniting-point, is also exhibited. The igniting-point concurs most with the specific gravity and the boiling point. This becomes particularly evident when the distillates of petroleum, boiling at a low point, are separately examined.

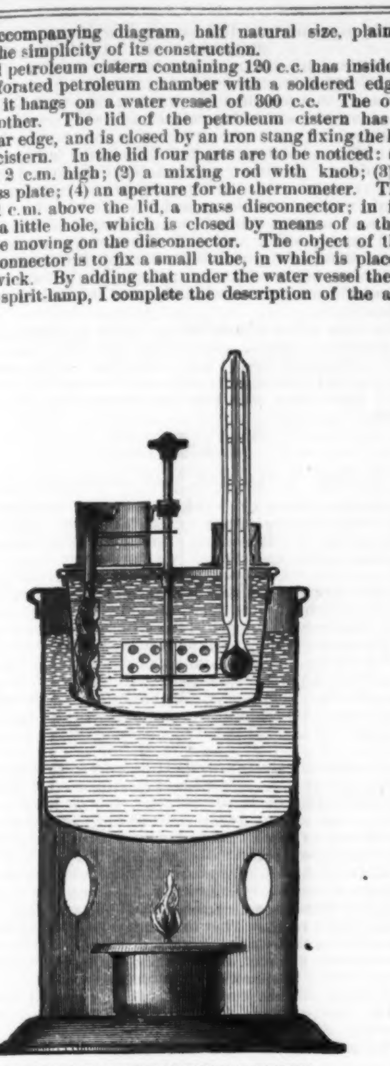
				Index of Refraction.
For the distillation	between	100°—	110° C.	1·3991
"	"	110	120	1·4033
"	"	120	130	1·4087
"	"	130	140	1·4101
"	"	140	150	1·4125
"	"	150	160	1·4151
"	"	160	170	1·4179
"	"	170	180	1·4205
"	"	180	190	1·4239
"	"	190	200	1·4279
"	"	200	210	1·4306
"	"	210	220	1·4332
"	"	220	230	1·4359
"	"	230	250	1·4395
"	"	250	280	1·4458

According to these examinations, petroleum, for which a specific gravity of above 0·800 and an igniting-point of 38° C. is sufficient, has an angle of refraction of 1·4480. With some practice the last examination is very simple, and requires a few minutes' time only. Most chemists are likely to prefer it to any other process, for, as it is, it gives a clew to find out its usefulness or its defectiveness. If, however, in examining petroleum its physical properties are entirely left aside, and importance is attached solely to its quality of igniting and burning, the apparatus must be self-acting and sufficiently safe.

From several hundreds of experiments I may perhaps infer that the many apparatus which have been in use up to the present day do not answer the purpose. I would not, therefore, venture to increase their number by one were I not induced to do so by several chemists of the province of Hanover, who for some time already have, to their greatest satisfaction, worked with my petroleum tester, and were I not convinced that this little apparatus—which, thermometer included, costs here in Hanover only 5 marks (5 shillings)—fully answers its purpose. The same advantage cannot be expected even from the best and most expensive apparatus known.

The accompanying diagram, half natural size, plainly exhibits the simplicity of its construction.

A small petroleum cistern containing 120 c.c. has inside a small perforated petroleum chamber with a soldered edge, by which it hangs on a water vessel of 300 c.c. The one fixes the other. The lid of the petroleum cistern has a rectangular edge, and is closed by an iron stang fixing the lid with the cistern. In the lid four parts are to be noticed: (1) A cap of 2 c.m. high; (2) a mixing rod with knob; (3) a small brass plate; (4) an aperture for the thermometer. The cap has, 1 c.m. above the lid, a brass disconnector; in its center is a little hole, which is closed by means of a thin brass plate moving on the disconnector. The object of the brass disconnector is to fix a small tube, in which is placed a cotton wick. By adding that under the water vessel there is a good spirit-lamp, I complete the description of the apparatus.



A SIMPLE PETROLEUM TESTER.

The petr[illegible] to be examined fills the vase up to the edge of the interi[illegible]hamber which is to receive the cotton wick. The lid being put on, the spirit-lamp is lit along with the little lamp in the cap. The knob is now to be turned from time to time, and the thin brass plate to be drawn out. Nothing else is required. In about ten minutes the examiner will have ascertained the inflammability (flashing-point) of the petroleum. For the very moment inflammable gases begin to issue through the aperture under the little flame they ignite, and the flame is extinguished with little loss. The spirit-lamp is then to be put out, the lid to be removed, and the thermometer to be placed in the petroleum chamber now prepared for it. Now and then a burning match is quickly brought in contact with the petroleum, and, after blowing it out, the igniting temperature is read, when the whole surface will be in a blaze and continue burning.

The experiments made with this apparatus by several chemists have produced the most favorable—and in each case invariable—results.—*Chem. News.*

THE ACTION OF CERTAIN ORGANIC SUBSTANCES UPON THE ROSE COLOR PRODUCED BY THE SOLUTION OF PLATINIC IODIDE IN POTASSIC IODIDE.

By FREDERICK FIELD, F.R.S.

SOLUTIONS of starch, cane and grape-sugar, gum, dextrin, gelatin, glycerin, nitrous and oxalic acids, and many other substances have no perceptible action upon the rose color, even when they are highly dilute. Neither has urea nor uric acid; but urine, albumen, tannic, gallic, and pyrogallic acids, potassic cyanide and sulphocyanide, and the liquids in which meat or vegetables have been boiled, destroy the color with greater or less rapidity.

It occurred to me that these facts might have some value, as, although the results were not conclusive as to the actual existence of any one body in the liquid, the *negative* result, in this instance, viz., the non-change of color, would be a tolerable certain evidence of the freedom of the added solution from substances possessing a deleterious influence. Thus, although the presence of gallic or tannic acids would cause the disappearance of the tint, albumen, urine, sewage water, and the water of wells near a cesspool, would have a similar effect. The resistance of the color to the influence of the liquid would prove the absence of all the above substances, whether baneful or harmless.

A solution was made containing one part of platinic chloride in 500,000 of a very dilute solution of potassic iodide. 50 c.c. of the liquid were placed in a series of wide glass test-tubes, and a similar volume (50 c.c.) of the following were added to each, and thoroughly mixed. The original solution had a clear bright rose color.

Water from the Thames taken at high and low tide at Lambeth.—No great effect. There was a simple dilution of color owing to the addition of the colorless liquids.

Water from the various London companies.—No perceptible changes, but in one or two instances the color faded considerably in the course of twelve hours.

Water from the main drainage, after the sewer had been recently "flushed."—Speedy decolorization.

Water from same sewer, having a slightly disagreeable odor.—Instantaneous destruction of color, even when diluted with ten times its volume of pure water, *i. e.*, 5 c.c. of sewage to 45 c.c. water.

Water in which vegetables and meat had been boiled.—Similar result.

1 c.c. urine added to 49 c.c. water destroyed the color after a few minutes.

1 c.c. solution of albumen prepared by dissolving the white of one egg in 200 c.c. water, immediately discharged the color from the platinum salt.

Dilute solutions of gallic, pyrogallic, and tannic acids.—Same reaction; as well as potassic cyanide and sulpho-cyanide.

Saliva also destroyed the color.*

On the contrary—

Solution of urea	No effect
" uric acid	"
" starch	"
" gum	"
" dextrin	"
" cane sugar	"
" grape sugar	"
" glycerin	"
" gelatin	"
" oxalic acid	"
" tartaric acid	"
" citric acid	"
" acetic acid	"
" carbon disulphide	"
" alcohol	"

From the above experiments it would appear that many organic substances which speedily decompose potassium permanganate do not affect the platinum salt. As it was thought that the platinic iodide employed was rather too dilute, 1 part in 200,000, in place of 1 in 500,000, was employed. This solution has a rich ruby color, and seems affected in the same degree, though perhaps not quite so readily. The reaction of a solution of albumen is by far the most striking of any of the results mentioned above. The splendid red tint on the addition of a few drops of that liquid instantaneously disappears. This would be a good lecture experiment, although by no means conclusive of the existence of albumen.

Such are some of the observations which have been made, and must be regarded merely *as* observations, having, perhaps, no great scientific interest in themselves. It may be, however, of some importance to the chemist to know that an equal volume of water added to a solution of platinum iodide of the strength above given, and retaining its color, may be regarded as comparatively free from sewage contamination and from albuminous matters, although it may contain nitrites and nitrates. The water from wells, unfortunately near a cesspool, could thus be easily tested. The destruction of color *might* give a false alarm; the retention of tint would, to say the least, be consolatory.—*Chemical News.*

* I am not aware that it is generally known that iron cannot be recognized in urine (the metal as a ferric salt) by means of potassic sulphocyanide, unless excess of hydrochloric acid is present. This must be due to the phosphates, which destroy the red color. Potassium sulphocyanide exists in saliva, and it is a well-known experiment to add a drop of ferric chloride to that secretion and mark the characteristic tint. If a trace of sodic phosphate be present there is no change of color, which is developed, however, on the addition of hydrochloric acid.

ON OZONE.

HAUTEFEUILLE and CHAPPUIS have further studied, by means of the spectroscope, the formation and destruction of ozone with the following results: The absorption bands of ozone, pure and dry, and formed from oxygen free from nitrogen, disappear slowly at ordinary temperatures, rapidly at red heat; and this takes place whether the ozone is inclosed in a vessel or is in the form of a gaseous current, the spectrum finally becoming continuous without the appearance of any new lines. The same phenomenon is observed in the destruction by heat of ozone mixed with nitrogen, the latter not having been exposed to electrical influence. There is no indication of the formation of hyponitric acid. It is admitted that in ozonizing oxygen in the presence of nitrogen at low electrical tensions, no hyponitric acid is formed. But the authors find that in ozonizing at ordinary temperatures a mixture containing one-seventh nitrogen, there is always produced a new substance, indicated by remarkable absorption bands differing from those of electrified nitrogen, of nitrous, hyponitric, and nitric acids. If the gas be passed through water the latter becomes acid, and the gas shows the spectrum of ozone only. So, too, if the gas be not dry, the new lines rapidly disappear. The same gaseous mixture is quickly destroyed at red heat, the bands of ozone and of the new substance disappearing, and those of hyponitric acid appearing and persisting. If the slow decomposition at ordinary temperatures be followed with the spectroscope, it is seen that the new bands disappear in from 24 to 48 hours before there is a trace of hyponitric acid. The latter then slowly appears; from which the authors conclude that the new substance decomposes into oxygen and nitric anhydride, and the latter into oxygen and hyponitric acid. Berthelot had observed that a mixture of hyponitric acid and oxygen subjected to electrical influence became colorless, and the authors, in repeating the experiment, find in the resulting mixture the new substance, but no hyponitric acid. They suggest the probable existence of a pernitric acid analogous to the persulphuric acid discovered by Berthelot.—*Comptes Rendus.*

ON NITRIFICATION.

SCHONBEIN attributed the formation of nitrates to the union of ozone and nitrogen, but Berthelot has stated that this union does not take place directly. Hautefeuille and Chappuis have shown, however, that it does take place under the influence of electricity of low tension with the production of a new substance, probably pernitric acid. The gaseous mixture was subjected to the influence of electricity of very low tension. The ozone was so diluted that it did not show absorption bands in a column two meters long, but there was a suspicion of bands of pernitric acid. When the current was heated with an alcohol lamp these appeared distinctly. The electrical tension was then increased as much as the apparatus would allow. The amount of pernitric acid formed increased progressively, but no hyponitric acid was obtained directly. The authors suppose that the formation of hyponitric acid, even at high tensions, may be due to the decomposition of pernitric acid by the greater heat of discharge.

Studying the effect of heat alone upon pernitric acid, they find that it is decomposed at all temperatures, but that at 130° the decomposition is completed in a few seconds, yielding oxygen and hyponitric acid. In order that the formation of pernitric acid should take place in the atmosphere, the presence of water vapor must be no obstacle. The authors found, however, that the absorption bands of pernitric acid did not appear if the gas was not perfectly dry, but in this case liquid acid condensed upon the walls of the containing vessel. Thus by passing three liters of air with water vapor through the apparatus, they obtained 0·054 gramme of nitric acid. From these experiments it is seen that nitric acid may be formed under varying conditions by the union of oxygen, or ozone, and nitrogen under the influence of electricity of low tension, pernitric and hyponitric acids being intermediate products.—*Comptes Rendus.*

HOW WE ARE POISONED.

AT a recent meeting of the Lancaster, Pa., Agricultural Society, Dr. C. A. Greene read a paper on the above subject in which we find the following:

Thousands of persons die every year from poisons taken into the stomach. I propose briefly to show in what manner it is done, and also to show that thousands of persons also suffer pains, some of them almost indescribable, from the absorption of poisons into the body. On the outside of the body are millions of little holes called absorbents, which have the power like a suction-pump of drawing into the body almost anything that may come in contact with the skin. Hence it is a self-evident fact that under no consideration should poisons of any kind be handled nor should they be taken into the alimentary canal. The object of a man or animal's stomach and intestines is to convert food into blood, and any foreign substance in these organs acts (like a splinter in the flesh) irritantly. Hence they are contraindicated. Newspapers throughout our commonwealth often publish receipts and items on physiology that are truthless and worthless and often exceedingly injurious. In a March number of the Philadelphia *Record* sulphate of zinc and foxglove (or digitalis) are called a sure remedy for smallpox, and yet both of them are powerful poisons; one grain of foxglove, which is the 1-480th part of an ounce, has been known to produce vertigo, extreme pains, dimness of vision, and a reduction of the pulse from 80 to 40 beats a minute. In the same issue was the following receipt:

"A solution of oxalic acid is the best for scouring and polishing copper. Finish with whiting."

Now as editors are not chemists or physicians, why will they in this reckless manner give such statements to their readers? The blacksmith who never saw an astronomical instrument does not force his crude conceptions of celestial bodies upon the people. Oxalic acid is also a very dangerous poison, and only a few grains of it taken into the stomach will produce disastrous symptoms and death, and merely handling it may introduce into the system sufficient to produce thousands of unnecessary pains and aches. It should never be found in your home; it is as dangerous as a rattlesnake.

COPPER UTENSILS.

Many farmers do a large amount of cooking for themselves and their cattle, poultry, etc., in copper and brass kettles. Any of them when not used for a time are lined with verdigris, called in the books subacetate of copper, also oxide of copper, and it is soluble in water and is a virulent poison. Brass kettles are made from copper and zinc. Any acid will always act upon metals. If you stew apples, cranberries, tomatoes, or any fruit or vegetable that is of an acid nature, the acid eats or corrodes the copper or zinc and forms usually acetate of copper or zinc. No matter how small the quantity swallowed it is a foreign substance, as well as poisonous and produces indigestion. The acid of apples is called malic or sorbic acid, and if it comes in contact with copper, zinc, lead, or tin, will produce malate of copper, zinc, lead, and tin. The fermentation of apples or cider, made from apples, produces vinegar, which is dilute acetic acid, and it will also produce the same chemical changes if it has the opportunity, and the results will be acetate of copper, acetate of zinc, lead, or tin. When the milk becomes sour it produces lactic acid, which will act in the same manner as the two acids, and form lactate of copper, lead, zinc, and tin, and all of these metals are poisonous, and every one injures the health of the individual who has eaten them in his or her food. Dyspepsia in some of its forms, paralysis, neuralgia, and affection of the organs of the body, are the sequences. I would as soon have a copperhead snake in my house as a brass or copper utensil for cooking purposes. If they are scoured ever so clean, the acid will act upon them even more readily. It is a common occurrence when pickles become a little changed in the spring, to put pickles and vinegar in a copper or brass kettle and boil them for a time and they come out much improved in appearance and handsomely greened. This bright color is acetate of copper. Tin vessels also lose their luster by long exposure, and forms a poison called oxide of tin. Lead pipes have been used for many years to convey drinking water; if it stands for some time in the pipe the oxide of lead is formed, and any one drinking it is poisoned.

The quail and partridge in the cold winter months eat poison berries, and in this way they contaminate their flesh and injure the health of the one who eats it. Acetic acid is distilled vinegar. If you take one pint of acetic acid and seven pints of water, and unite together, you have eight pints of vinegar.

SOAP.

Some soap makers, regardless of the consequences, take the tallow or fat of diseased animals and make them into soap. The unchanged virus is absorbed into the body while being used for washing purposes. If you cook lemons in a brass or copper kettle, the acid of the fruit, called citric, will act upon the metals in the same manner and form citrate of copper, zinc, etc.

HAIR BRUSHES.

Many persons use the hair brush of another individual, or the barber uses upon a hundred or a thousand heads the same brush. If any of his patrons have tetter, eczema, syphilis, or other skin disease, it can be readily conveyed to any one whose head is briskly rubbed with it. In the above and many other ways are poisons conveyed into the body and the victim of the virus may suffer all his life from the effects. I have brought for inspection some of these poisons, and to show how small a quantity of copper will by the laws of affinity make itself known. I propose to add one drop of a solution of nitrate of copper to one hundred drops of water, and then add one drop of aqua ammonia to the colorless liquid, and it will at once become beautifully blue. I will conclude by saying that there is a friend of mine in this city who has over 100 tumors on his body occasioned by his handling paints.

At the close of his essay Dr. Greene made a number of chemical experiments with the poisons referred to in the essay.

Mr. Engle said it was news to him that the souring of milk in tin cans produced a poisonous acid, and yet there seemed to be no doubt it would do so.

In answer to questions, Dr. Greene said that tin was a less dangerous metal to be brought in contact with food than zinc, brass, or copper. Iron vessels may be safely used as cooking utensils, as when iron taken in proper proportions is not injurious; but people usually get enough of it in the food cooked in iron vessels, without taking it as a medicine.

Mr. Linville believed there was great danger of being poisoned by the use of milk kept in tin pans, and thought dairymen made a great mistake in substituting tin cans for the old-fashioned earthen crocks. If vessels of pure block tin were used and were kept scrupulously clean there might not be much danger in using them, but unfortunately, the so-called tinware contained a large proportion of lead which is much more readily decomposed by acids than tin and is also a much more virulent poison. He also spoke of the danger of poison from boiling apple butter in copper-kettles; and yet he and everybody else use copper-kettles for this purpose. As people will have apple butter, he advised that the kettle be scoured scrupulously clean; that the cider be immediately put into and heated as soon as possible, and that all the apple butter be removed from the kettle before it cools, as the decomposition of the copper and the formation of the poison goes on much more rapidly when the acid of the apple butter is cold than when it is hot.

ON THE ATTENUATION OF THE VIRUS OF CHICKEN CHOLERA.*

By L. PASTEUR.

OF the various results which I have had the honor of communicating to the Academy concerning the disease commonly called chicken cholera, I will take the liberty of recalling the following:

1. Chicken cholera is a virulent disease of the highest order.

2. The virus is a microscopical parasite, which may be multiplied by cultivation outside of the body of an animal.

3. The virus presents various degrees of virulence. Sometimes the disease is followed by death; at other times, after giving rise to morbid symptoms of variable intensity, these are followed by cure.

4. The difference noted in the power of various viruses are not merely the results of observations made of natural phenomena, for the experimenter can give rise to them at will.

5. As generally happens with virulent diseases, chicken cholera does not recidivate, or rather it may be said that recidivation is in inverse ratio to the intensity of the first attack of the disease, and it is always possible to carry the preservative action far enough to prevent the most virulent virus from producing any effect.

6. Without wishing to affirm anything at this time on the relations of the virus of variola to that of vaccine, there appears from the foregoing that in the chicken cholera there are conditions of the virus which, relatively to the most virulent virus, act as human vaccine to the virus of variola. Vaccine virus gives a mild disease, but preserves from a more serious disease, variola. In the same manner the virus of chicken cholera presents, in certain conditions, an attenuated virulence which gives the disease but does not cause death; and after cure has been effected, the animal may without danger be inoculated with the most virulent virus. There is, however, a great difference between these two sets of facts, and it must be acknowledged that in some respects the advantage is with investigations relating to chicken cholera, as far as knowledge and principles are concerned; for while discussions continue on the relations of vaccine to variola, we possess the assurance that the attenuated virus of chicken cholera is derived from the very virulent virus proper to this disease, that we may pass directly from one form of the virus to the other. This fundamental nature of each is the same.

The time has come for me to give explanations concerning the main fact of the preceding propositions, which is that there are variable states of virulence in chicken cholera. This must certainly seem a strange result if we bear in mind that the virus of this affection is a microscopical organism, which may be cultivated in a state of perfect purity, as might be done with beer yeast or with mycoderm of vinegar. If we reflect on this mysterious problem of variable virulence, we are led to think that this characteristic is probably common to the various species of this group of virulent diseases. Where is, then, the distinctive character which belongs to these diseases? To cite only one example: We often see very severe epidemics of variola, and others of a milder type, without the possibility of ascribing these differences to the variable conditions of climate or of individual constitutions. We also see great contagia become gradually extinct, to reappear again and again to become extinct.

The idea of the variable virulence of the virus is not, then, of such a nature as to surprise physicians and educated persons, but nevertheless it becomes interesting to establish it on scientific bases. In the particular case we have before us the mystery lies in this, that the virus is a microscopical organism, and that the degree of virulence depends on the observer. This I mean to establish beyond doubt.

Let us take as starting point the virus of cholera in its most virulent form. I have already given a process for obtaining it with the maximum of virulence, which consists in taking it from the chicken that has recently died, not from the acute disease, but from the disease in its chronic form. This form sometimes presents itself, although rarely. The chicken resists the disease for weeks and even months. When it has died, which happens when the parasite, after being localized, finally passes into the blood, we may observe that whatever may have been the virulence of the virus originally inoculated, the virus from the blood of the animal who has taken so long to die is of such virulence that it kills in every case. If we make successive cultivations of the virus obtained in this way, in a pure state, in a broth made from chicken's muscles, by starting each cultivation from the preceding one, and if we make trials from each successive cultivation, we will find that the virulence does not vary in an appreciable degree. In other words, if we make this convention, that two cultivations have the same degree of virulence when on operating in the same conditions on animals of the same species the proportion of deaths is the same, we say that in our successive cultivations the virulence remains the same.

In what I have said I have not made any mention of the interval of time between the beginning of one cultivation to the beginning of the next, and of the possible influence of this interval on the degree of virulence. I will now call your attention to this point, although its importance may appear small. For an interval of from one to eight days, the virulence does not seem to change. For an interval of fifteen days, we have the same result. For an interval of a month, of six weeks, or of two months, there seems to be no diminution of virulence. Nevertheless, as the interval becomes greater there appear signs of little apparent value that the virus has become weaker. For instance, although the proportion of deaths remains the same, the rapidity with which chickens die does not seem to be so great. In the various series of inoculated chickens, some seem to linger, although very ill and sometimes very lame, as the parasite has settled in their thighs. The pericarditis is of milder type, and abscesses occur around the eyes. The disease seems to have lost its overwhelming character. We may make the intervals still greater: we may put three months, four months, five months, eight months between two successive cultivations. Then we obtain entirely different results, for the degrees of virulence, which formerly were not perceptible, now become well marked by apparent effects.

By such long intervals between successive seminations, it happens that the next cultivation does not present mortalities of ten for ten inoculations, but decreasing mortalities of nine, eight, seven, six, five, four, three, two, one for ten inoculations, and sometimes there are no deaths, which means that the disease shows itself on all the subjects inoculated, and they all recover. In other words: by simply changing the process of cultivation of the parasite; by merely placing a longer interval of time between successive seminations, we have obtained a method for decreasing virulences progressively, and finally get at a vaccinal virus which gives rise to a mild disease, and preserves from the deadly disease.

We must not think that for all these attenuations things proceed with mathematical fixedness and regularity. A cultivation which has stood for five or six months without renewal may show remarkable virulence, while another may be considerably attenuated after waiting three or four months. We will very soon explain these irregularities, which are only apparent. Often even there is an abrupt passage from one condition of notable virulence to the death of the parasite for an interval of short duration. In passing from one cultivation to the next we are sometimes surprised to find that further development has become impossible. The parasite has died. The death of the parasite is an event which often occurs when sufficient time has elapsed after a new semination has been begun.

Now this Academy understands the true reason of the silence I have kept, and of the liberty I asked to delay information on my method for effecting the attenuation of the virus. Time was an element in my researches.

While the various phenomena are taking place, what becomes of the microscopical organism? Does its shape and aspect change, while its virulence is changing in such a marked manner? I would not dare to affirm that there does not exist certain morphological relations between the parasite and the various degrees of virulence which it shows; but I must confess that it has not been possible for me to seize them. If any such relation sometimes appear, they disappear again to the eye working through a microscope, on account of the extreme minuteness of the virus. The cultivations seem to be the same for all degrees of virulence. If sometimes slight changes are seen they seem to be entirely accidental, for in the next cultivation they either disappear or even sometimes inverse changes take place.

A remarkable circumstance is that if we take each variety of virulence as a starting-point from which to make new cultivations, at short intervals, each variety of virulence keeps its own intensity. If, for instance, we have an attenuated virus which can only kill once in ten times, it will keep the same degree of virulence in its successive seminations if the intervals are not too great. A very interesting circumstance, although in accordance with the preceding observations, is that an interval of semination which may be sufficient to cause the death of an attenuated virus has no sensible influence on a more virulent virus, although this may in its turn, become afterwards attenuated to a marked degree.

Now that we have arrived at this point, a question presents itself which relates to the cause of the attenuation of virulence.

The cultivations of our virus must take place in contact with air, because our virus is aerobian, and without air, its development becomes impossible. We are then naturally led to ask whether the attenuation of the virus is not due to contact with the oxygen of air. Would it not be possible that the small organism which constitutes the virus, when left in contact with the oxygen of pure air, in the medium of cultivation in which it has developed, may have been modified, and the change remains permanent, even after the organism has been withdrawn from the modifying influence? We may also inquire whether some chemical principle in the atmosphere, other than oxygen, does not intervene in this phenomenon, the singularity of which almost justifies my hypothesis.

It is easy to understand that the solution of this problem, in case it depends on our first hypothesis, that the phenomenon is due to the oxygen in the atmosphere, may be tried by experiment. If oxygen is in reality the cause of the attenuation of virulence, we may have, to a certain degree, a proof of it by noting the effect of suppressing it.

To test this, let us conduct our cultivations in the following manner: We may take a certain quantity of our chicken broth, and place it in the most virulent virus, and fill with it a series of glass tubes up to two-thirds, three-quarters, etc., of their volumes. These tubes may then be closed over the lamp. By the presence of the small quantity of air left above the liquid the development of the virus may be started, which is ascertained by the increasing turbidity of the liquid. The development of the cultivation gradually absorbs all the oxygen contained in the tube. The turbidity then diminishes, the growth is deposited on the sides of the tube, and the liquid becomes limpid. This takes place generally in two or three days. The microscopic organism is then deprived of oxygen, and will remain in this condition as long as the tube is not opened. What will become of its virulence? To be sure of our results we will have prepared a great number of such tubes and an equal number of flasks, which last will continue to be left in contact with pure air. We have already spoken of what becomes of cultivations carried on in presence of air. We know that they experience a progressive attenuation of their virulence, and we will not return to this subject. Let us now only pay attention to the cultivations in closed tubes. Let us open them—one after an interval of a month, another after three months, and so on until we open one that has stood ten months. I have not gone any further at the present time. It is a remarkable circumstance that the virulence in all these cases is of the same degree as that of the liquid which served to fill up the tubes. As to the cultivation exposed to the air they are found either dead or in a condition of feebler virulence.

The question we have proposed is then solved: it is the oxygen of the air which attenuates and extinguishes the virulence.

To all appearances we have here what is more than an isolated fact. We must have reached to a general principle. We may suppose that an action which is inherent to atmospheric oxygen, an agent present everywhere, has the same influence on other viruses. At any rate it is worthy of interest that possibly a general cause of attenuation exists dependent on an agent which is in a manner cosmical. Can we not suppose even now that it is to this cause that we can attribute in the present, as in the past, the limits set to great epidemics?

The facts which I have had the honor to communicate to the Academy suggest many proximate and remote inductions. From all these I must hold back with reserve. I will not feel authorized to present them to the public, unless I make them pass into the domain of demonstrated truths.

* Translated from *Comptes Rendus de Academie des Sciences* of October 25, 1880, by P. Casamajor.—*Chemical News.*

MICROPATHY.

By JNO. W. MACLEAN, M.D., Washington, D. C.

FROM the earliest record of the history of medicine, to the present day, in one branch of the art there has been no change—that of administering remedies. When Hippocrates announced that disease must be overcome by opposites and be driven out by violence, he gave the keynote to all the differing forms of practice which have flourished from that period until the present day. This idea of shock or violence being necessary to conquer disease, is the origin of most of the objections which are brought forward against the profession of medicine, as no physician is able to exactly foretell how a particular remedy will act upon a patient, owing to the numberless peculiarities of constitution and temperament, therefore the use of drugs has become a plan of experiments; this has led to a general doubt, in regard to the action of remedial agents, in the minds of physicians, owing to the failures and disappointments which these experiments have caused. This doubt has caused the profession to be divided into so many different schools and forms of practice, but, although they differ as to their practice, agree as to this experimental plan of giving medicines. A want of faith has also spread to the minds of the people, who have lost all confidence in any particular form of practice, and are prepared to trust any charlatan who can tell a plausible story, and to use secret remedies, which are only another form of experiment. That there are certain fixed properties of medicines, that we do possess qualities, in these agents, on which we can rely, that certain quantities will produce certain effects, the practice of micropathy will prove. This word (from mikros, small, and pathos, disease) is a term used to represent the tonic action of remedies, in small quantities, which theory was first discovered by Jno. A. Maclean, M.D., of Norwalk, Conn., about 1830. Dr. Maclean was attending a case of vomiting, in which all the ordinary remedies had failed. Upon giving one-sixtieth of a grain of tartar emetic, every fifteen minutes, he found that it acted as an irritant to the organ affected; he then reduced the dose to the one-hundredth of one grain, given in the same manner, and soon saw that the disease was under control, which continued until the patient was convalescent. From this time his practice was a succession of experiments to establish the truth of this theory, and he soon demonstrated that whenever a remedy irritates an organ, by reducing the dose to a certain point, it will act as a tonic to that organ. This certain point, roughly stated, is about one-hundredth of the ordinary allopathic dose, that is, if the allopathic dose of rhubarb is ten grains, the micropathic dose is one-tenth to one-twelfth of a grain, but if this dose causes any irritation, it is a symptom that the quantity given has gone beyond the tonic action and must be reduced, and as the patient is always warned of this action, he can always regulate the quantity taken, according to the symptoms. On carrying this principle into his practice, Dr. Maclean soon proved that dyspepsia, heart diseases, female complaints, nervous maladies, disorders of the kidneys and bladder, hemorrhoids, and in fact, nearly all of the chronic diseases, so many of which are declared incurable by the profession, are amenable to the tonic action of drugs, and we have numerous witnesses to prove the assertions which we have made concerning these diseases. We claim for micropathy:

1st.—That it is much more exact, for we can prescribe for 100 patients, with the same disease, with only an accurate description of their symptoms, and will cure the great majority of these cases.

2d.—That it is much safer, for as soon as a remedy causes any irritation, it is a symptom that the dose given has gone beyond the tonic action, and therefore must be reduced, and as our patients are warned of this effect it is almost impossible for an overdose to be given.

3d.—It will bring into use hundreds of remedies which have been thrown aside as inert and useless, owing to their inability to produce the violent action looked upon as a desideratum in the action of medicine.

4th.—It will explain the action of medicinal springs, which has so long puzzled the profession, for the quantities of salts in these waters agree almost exactly with the micropathic formulæ of the same remedies.

5th.—We can prove by our practice, that these chronic maladies are curable, for we have cured by the aid of micropathy.

A CURIOUS RESULT OF MENTAL DERANGEMENT.

DR. BEARD explains the marvelous powers exhibited at times by subjects in a mesmeric or trance condition by the exaltation of one faculty while all the rest are for the time suppressed. A permanent exaltation of one faculty—as of calculation, music, or other—with the more or less suppression of all other faculties, is sometimes seen in idiots, whose brain force is apparently confined wholly to one line of effort. In such cases any improvement in general ability or intelligence is usually attended by a falling-off in the exalted faculty. A reversal of this order appears in a remarkable Russian subject, lately brought before the Medico Surgical Academy of St. Petersburg. The subject, a man of twenty-seven years old, was in youth noted for brilliant abilities. A course of dissipation ended by an acute disease, on his recovery from which he was found to have lost all his mental faculties except memory and the power of mathematical calculation. Those increased proportionately as his understanding and power of logical thinking vanished. Now he is a living phonograph and calculating apparatus.

lecture room Professor Merjevsky requested him to square numbers containing five or six figures, to extract the square root of like numbers, and so on. All the questions were correctly answered by the patient in a few seconds. No mathematician present could do anything like it. Then the professor requested some one to read poetry aloud for several minutes, and the patient repeated it as correctly as a phonograph. The professor declared to the audience that he was unable to explain this psychical phenomenon. The memory and calculating capacity of the patient are still growing, while in other respects he is becoming a more hopeless idiot.

THE FERNS OF THE PACIFIC COAST.*

THE ferns of the Pacific coast, found growing nowhere else in the whole world, except as exported or cultivated, represent twenty species and four well-marked varieties. The number found on the Pacific coast, as well as elsewhere—*i. e.*, the cosmopolites—are forty-two species and fourteen distinctly marked varieties; adding these to the exclusively Pacific coast ferns, we have in all sixty-two species and seventeen varieties.

In all North America, north of the Mexican boundary, there have been discovered up to date one hundred and fifty-one species and twenty-four varieties. From this, it will be observed, the Pacific coast claims more than one-third of the entire number. With such a large proportion of this interesting and most beautiful division of the vegetable kingdom represented, it seems fitting, as we have attempted, to segregate from all directions, and put the material into compact and available form. This has been done with painstaking, and no little research, compiling from the latest authorities; the newly-discovered ferns being described from personal observation, aided by the microscope, upon the living plants and the dried specimens of the herbarium.

Let us touch briefly, as the hour permits, upon the geographical range of ferns; their origin, life, history, some peculiarities, their brief American literature, and lastly, in a general way, tell something about the latest discovered ferns, together with the uses of ferns in general.

GEOGRAPHICAL RANGE.

Ferns generally love heat, shade, moisture, and stillness; hence, are most abundant in the islands of the tropics, but they are distributed over all quarters of the globe, always in much smaller ratio than the flowering plants.

The whole number of species known and described up to date is about three thousand. Four hundred and sixty species are found in the single island of Java; the small island of Ceylon has two hundred and fourteen species; the West Indies, in round numbers, has four hundred. On the mainland, Brazil has three hundred and eighty-seven; the Isthmus of Panama nearly one hundred and twenty; while tropical America reaches the large number of nearly one thousand. Contrast with these regions North America, north of Mexico, with its one hundred and fifty-one species; all Europe, sixty-seven; and the Arctic zone, only twenty-six species.

The ratio of ferns to flowering plants may be readily seen by citing two or three examples:

Tropical America has one fern to thirty-five flowering plants.

New Guinea has one fern to four flowering plants.

United States (east of Mississippi) has one fern to forty-six flowering plants.

FLORAL DIVISIONS.

North America presents four natural floral divisions—the Atlantic slope, the Valley of the Mississippi, the Rocky Mountain region, and the Pacific slope. When we speak of the Pacific coast in this paper, let it be understood that we mean a *naturally* distinct division, bounded on the south by Mexico, on the east by a line through the mid-valley regions, lying between the Rocky Mountains and the Sierra Nevada range, extending northeastward to Alaska. This general division will be found to include the southwest corner of New Mexico, all of Arizona, the southwest portion of Utah, all of Nevada, California, Oregon, Washington Territory, British Columbia, and southern Alaska. This gives a wide range of temperature; from the hot tropical and forcing, through the milder temperate, to the dwarfing cold of the Arctic regions. All these conditions are, also, produced, of course, by difference of altitude, from the humid coast to the ever-snow-clad peaks of the region.

Special climatic conditions are the result of peculiar trends of coast and mountains, producing special flora. For example, the large number of plants, including the great sequoias and Santa Lucia, or Bracted fir, foundonly in limited localities of this region.

Ferns are always associated with ideas of shade, coolness, shelter, and protection, mostly nestling around rocks or clinging to trees, filling up the shady interstices. There is, however, one exception to this law, which is very notable. A so-called variety of *Pellæa wrightiana* is found on Mount San Bernardino, growing out in open sunny slopes, like a *Delphinium*, or larkspur, the Salvia, etc., and for this reason, combined with others, based upon contracted fronds, ashen hue, extreme rigidity, etc., we believe it to be distinct.

PECULIARITIES OF DISTRIBUTION.

Sometimes identical species appear in the most widely-separated regions. An illustration may be given of one of our own ferns, *Aspidium mohrioides*, known as the "Falkland Islands Shield fern," first discovered on the Falkland Islands in 1824, by the botanists of Duperry's voyage, later at Patagonia and in the mountains of Chile; all the while limited to the southern part of South America and its adjacent islands. Now, away up in northern California, near Mount Shasta, six thousand miles from its nearest known habitat, Mr. Lemmon discovered, in 1879, this beautiful fern, which was at first supposed by Prof. Eaton to be a distinct species. It excited such attention that a council of distinguished pteridologists assembled, and, after due examination, felt obliged to consider it identical with this Falkland Island fern; but as it is a magnificent fern, new to North America, and found near the matchless Shasta, we give it the popular name in the catalogues of "New Shasta fern."

All but one species of ferns are terrestrial. This one (*Ceratopteris thalictroides*) is aquatic, and is found in the everglades of Florida, the sterile frond floating on the water.

All but a few species are perennial, and nearly all of these, particularly the tree-ferns, are evergreen.

THE ORIGIN OF FERNS.

The first information we gather about the origin of ferns is recorded in a portion of the big book of nature—the everlasting rocks; not inscribed in hieroglyphics or shadowed hints of fern existence, but bearing the whole plant, or parts of it, entombed in their embrace.

In the Devonian age, or age of fishes, ferns first appear, rapidly increasing both in number and size, up to the Carboniferous age, when we find them so abundant as to constitute often the bulk of certain coal strata, and to have been tree-like in form, fifty to seventy feet in height, with trunks a foot to three feet in diameter. They attain their best development, also, in the Carboniferous period, and from the fossilized specimens of parts of the trunks of the magnificent tree-ferns, their cellular structure is shown in a wonderfully beautiful manner. After this period it seems that the luxuriance of vegetation diminished, as if the earth, in its over-feeding of these beautiful cryptogams, had become exhausted, so that one form after another disappeared, and the size of the remaining ones was greatly diminished, until we find them, as at the present age, with no apparent change since pre-historic days.

THE LIFE OF A FERN.

The life of a fern begins from a spore, which is analogous to the seed of a flowering plant, and tends to the same result—the propagation of its kind. In structure a seed and a spore are quite different; a seed having a definite rudimentary plant, while a spore contains nothing inside its cell-wall but a particle of protoplasm or albuminous substances, with no vestige of a plant, no matter how highly magnified; hence, we are prepared to find that the growth of a fern must be entirely different at the outset from that of the flowering plant.

FERN SPORANGIA.

These spores are always in little cases called *sporangia*, located on the back or underside of the frond. They are developed, in all instances, from the outer cells of the frond, upon which they are borne. The leaf tissue is often sacrificed to such an extent that the frond becomes greatly contracted, or the leafy portion entirely disappears and gives place to a mass of spore-cases, held together by the veins and skeleton of the frond, as in *Cryptogramme acrosticoides*.

When the numberless minute spores are ripe, the annulus, or ring that surrounds the sporangium—being made of firmer material than the netted or reticulated case—springs open, rending the tender netted case at the sides, and the freed spores fly off into the air like dust, depositing themselves in the earth, in crevices of rocks, and on tree trunks, or wherever the conditions are favorable for growth—that is, sufficient warmth, moisture, and stillness. After a few days a greenish scum or film covers the damp surface. This is the first stage of fern life.

CELL WALL OF THE SPORE.

The outer cell wall of the spore is composed of a peculiar substance called cellulose, composed of $C_6H_{10}O_5$. This cell is broken by the warmth and moisture; the protoplasm, or cell contents, comes out and divides, forming minute new cells, which join themselves to the first cell, and continue the process until little shield-shaped structures appear, or are built up, standing at an angle of about 45°, growing thick together, resembling small scales, imbricating or overlapping, like the scales on a butterfly's wing, or, if magnified, would resemble the slate upon a roof. These minute scales (*prothalli*) are attached to the earth, a damp well, rock crevices, or any favorable substance, by root-hairs, not true roots.

This first bed or prothallus, or pro-embryo, is composed of cells filled with a green substance called chlorophyl which gives it the usual bright, living green color. On the *under* side of this *pisthallus* are organs analogous to the stamens and pistils of flowering plants, and are called, respectively, *antheridia*, containing the male element, equivalent to the pollen of flowers, and the *archegonia*, equivalent to the pistil and ovaries of the flowering plant. The *archegonia* are near the *sinus*, or upper notched edge of the *prothallus*, located just above the *antheridia*.

When these two different cells open, the ciliated *antherozoids*, being endowed with elasticity and motile power, uncoil, swarm out, and some of them fall into the open cell of the tube-like *archegonia*, and at its base is located the mother-cell, called *oosphere*, or egg-cell, which it comes in contact with when the *archegonia* immediately closes. Its hidden process of development goes on, and at length it is found that the central cell divides into four cells, the two lowest sub-dividing; then become embedded in the substance of the prothallium. The two upper of the four cells subdivide also, one developing into the plumule or first stem, the other into a radicle or first rootlet of the young fern. And so it springs into life—a fine example of what is termed alternation of generation.

ANOTHER METHOD OF FERN GROWTH.

Ferns are also reproduced by means of *gemmæ*, or what is called *viviparous buds*, which grow on the stalk or surface of fronds; sometimes on the upper surface, as in *asplenium bulbiferum*, sometimes on the under surface, as in *cystopteris bulbifera*. In the *cystopteris* they fall off and grow during the second season, but in most others remain and develop several fronds, still drawing sustenance from the parent plant.

This curious method of growth is traced through all plant life, from the lowest *Algæ* to the highest *phanerogams;* especially does the tiger lily furnish striking examples in its detached axillary buds at the base of each leaf.

SOME PECULIARITIES OF THE FROND.

The frond or leaf of the fern seems capable, in some instances, of indefinite development in extension, or wonderful stretching outgrowth in the length of frond. Some show a dichotomous tendency of growth, *i. e.*, forking at the tips of the frond. Sometimes the fronds develop a yellow or white farinaceous powdery substance, usually upon under side, often in such abundance as to hide the delicate fruit and give name to the fern—as the gold back Cal. fern (*Gymnogramme triangularis*) or silvery plume fern (*Notholæna lemmoni.*)

FERN LITERATURE OF AMERICA.

Not till after our country had dated its century did any systematic work upon ferns, scientific or popular, appear from our American press. Much has been done abroad in reference to our ferns by Sir W. J. Hooker; and later, his son, Sir Joseph D. Hooker, with several others, has continued the work in bringing out general reports and descriptions of ferns, and the rest of *Vascular cryptograms*. Since that time, in the year 1878, Prof. J. Robinson has issued a neat little work, "Ferns in Their Homes and Ours," full of interest, and a valuable help to those who wish to cultivate these most graceful plants. Then followed an octavo volume on the "Ferns of Kentucky," illustrated with etchings from nature, by Mr. Williamson. Following close upon these came a check list of ferns on a single sheet by Mr. William Edwards, of Natick, Mass., intended as a convenient medium of exchange.

In 1879 Mr. George E. Davenport published, under the auspices of the Massachusetts Horticultural Society of Boston, a valuable and most instructive catalogue, or it might be called commentary, of some forty pages, relating to the Davenport herbarium of North American ferns. In 1880, "A Systematic Fern List of the Known Ferns of the United States of America, with the Geographical Range of the Species, and the Recognized Authorities for Nomenclature," was issued by Prof. D. C. Eaton, as an accompaniment to his large, exhaustive, and finely illustrated work on "Ferns of North America." This comes to us in two large octavo volumes of over six hundred pages—not mere picture books of ferns, but they stand in the first rank as a work of close, scientific research, a very present help to a close study of the ferns of our Pacific coast. The results of these studies it becomes a real pleasure to report.

NEW FERNS OF THE PACIFIC COAST.

The new ferns, or those but lately discovered and described, are ten in number:

Notholæna newberryi, Eaton.
Notholæna grayi, Davenport.
Notholæna lemmoni, Eaton.
Notholæna nivea, Desvereux.
Cheilanthes wrightii, Hooker.
Cheilanthes viscida, Davenport.
Cheilanthes cooperæ, Eaton.
Cheilanthes clevelandii, Eaton.
Aspidium nevadense, Eaton.
Aspidium mohrioides, Bory.

Three of these, *Notholæna grayi*, *Notholæna lemmoni*, and *Notholæna nivea*, were only detected last season in Arizona, and hence are not described and illustrated in Eaton's "Ferns of North America." These, with the peculiar and hardly less interesting *Aspidium mohrioides*, will be described in a general way in conclusion with the uses of ferns.

The *Notholæna grayi*, *lemmoni*, and *nivea* are all small, delicate, and fragile, white-powdered beneath, growing in nearly the same locality—Mt. Santa Catarina and Mt. Graham, southeast Arizona. The two first are plume-like, the latter pyramidal in outline.

The *Notholæna grayi*, Davenport, is a beautiful little fern, growing from four to six inches in height, is broad-lance shaped in outline, but simulating a plume tip. The stalks are few in number, and rise from a knobby or nodose root-stalk, growing in clumps on the grassy slopes of the foothills, under the shade of rocks. It was found many years ago by Mr. Schott, in Sonora, Mexico, but owing to the meager, fragmentary specimens—it being fragile and difficult to preserve—it was supposed to be portions of some other fern, and so was passed by, till again collected within the boundary of North America, by Wm. M. Courtis, in February or March, 1880, in southeast Arizona, the exact locality not reported; and by Mr. Lemmon, in April of the same year, in Sanoita valley, Patagonia mountains of southern Arizona. Larger and finer plants were collected in August on the foothills of Mt. Graham, near Camp Grant, southeast Arizona. Mr. Davenport describes it as a lovely fern, and quite different from any known species, and so cannot be compared. Under the microscope the white powder separates into distinctly stalked, gland-like bodies, with enlarged, conical, flat, or inverted heads, like a miniature host of fungi, with their variously shaped cups. The little brown scales that, with the powder, give it such rich color, under a power of two hundred diameters, become like long, tapering tubes, which contain the brown coloring matter, which, collected at the base, or at intervals throughout the scale, gives it the appearance of being jointed. It is a beautiful object for the microscope. Mr. Davenport concludes: "This species is one of the most elegant yet discovered, and I take pleasure in dedicating it to one pre-eminent in American botanical science—Dr. Asa Gray."

NOTHOLÆNA LEMMONI, EATON.

(Lemmon's silvery plume, *Notholæna*.)—During the same exploring expedition Mr. Lemmon fortunately detected a beautiful, silvery, plume-like *Notholæna*, appearing unlike any he had ever before seen. With eagerness he secured all the specimens possible, together with a few live roots from among the clefts of granite rocks. Its known habitat is in two ravines on the southern side of the Santa Catarina Mountains, at an elevation of about six thousand feet and about eight miles from Fort Lowell. These fragile but carefully prepared specimens were sent on to Prof. Eaton, and he at once replied: "Your No. 15 appears to be a new *Notholæna*." In the next issue of the Bulletin of the Torrey Botanical Club, June, 1880, he publishes a description of the fern under the above name. The close botanical description is also in its classified place with our Pacific coast ferns.

NOTHOLÆNA NIVEA, GILLIES.

(The snowy *Notholæna*.)—In the following month, the same party, while at Tombstone mines, searching along the granite comb above, regardless of the millions of rich quartz beneath his feet, discovered on the granite eminence, at about six thousand feet elevation, a delicate, snowy *Notholæna*, which, upon close inspection, proved to be the *Notholæna nivea*, its appearance true to its name. It was first discovered in Mexico, and as far south as Peru, but this is its first recognized welcome to our own land.

It is a very pretty and interesting fern, with prim, black, wiry, and polished stalks supporting its pyramidal fronds, that thus stand rather proudly, beckoning, as it were, to the prospector, and hinting, by its silver pinnules, flecked with golden fruit, that untold treasures of silver and gold are hidden in the silence of the rocky bed beneath. This species is also technically described in its proper order of analysis of the ferns of the Pacific coast.

ASPIDIUM MOHRIOIDES, BORY.

(The new Shasta shield fern.)—We come finally to speak of the most interesting discovery in ferns that has occurred for many years—the magnificent evergreen, full-fruited *Aspidium*, before spoken of as being found only at two widely removed stations on the globe, the southern part of South America and here, six thousand miles distant in Northern California, near Mt. Shasta.

This fern slightly resembles full-fronded specimens of *Aspidium aculeatum*, variety *scopulinum*, but is of a brighter, richer green, with pinnatifid or many-winged pinna; its fruit abundant, and so crowded upon the back of the frond that the very large covering over the spore cases or fruit are

* Read before the Academy of Sciences, San Francisco, by Mrs. S. A. P. Lemmon.

lapped or imbricated, like saucers on a sideboard; an appearance that is preserved in many of the herbarium specimens to a remarkable degree.

On the 8th of July, 1878, Mr. Lemmon discovered this fern near the headwaters of the Sacramento river, on the south side of Mt. Eddy, twenty miles west of Mt. Shasta. It grows around granite bowlders in moist places, accompanied by the varieties of *Aspidium aculeatum*, the *Scopulinum*, or "little brush fern." This association and close resemblance of the two ferns, has, no doubt, caused the escape from detection heretofore.

This circumstance of location also opens the door to a wide field of inquiry, as to why these closely resembling yet structurally different forms should be found in juxtaposition. Which is the pioneer possessor of the soil? Which is the usurper, simulating the livery of the rightful heir, and encroaching upon his domains? Or is there some subtle power in the elements of earth and air generated or tempered by the proximity of the lofty Shasta, that modifies and blends these gentle, passive ferns into almost like forms?

LITTLE KNOWN FERNS.

There are several little known ferns, found at long intervals of time by some especially sharp eyed explorer—perhaps seen only once along the Mexican boundary. In some instances the specimens are meager, being but a single frond, or only a few segments of a frond. Of these, Prof. Eaton entertains the hope that some one will be so fortunate as to find some of the ten at least, or, as he adds, rediscover the *Notholæna tenera*, the tender little cloakless wanderer; *Cheilanthes microphylla*, the small-leaf lip fern; *Cheilanthes leucopoda*, the white-stalk lip-fern; *Pellæa pulchella*, the most beautiful little cliff-brake; *Pellæa aspera*, the rough cliff-brake; *Adiantum tricholepsis*, the silky-leaf maiden hair; *Adiantum tenerum*, the tender-leaf maiden hair; and *Asplenium septentrionale*, the northern spleenwort—all the above-named being reported from the southern boundary. In the other direction, among the islands of Alaska, the *Cheilanthes argentea*, of Hooker, the silvery lip-fern, is supposed to be found, as it is abundant on the northern coast of Asia.

This subject about canvasses the subject of our Pacific coast ferns, and we will close by brief reference to the

USES, BENEFITS, ETC., OF FERNS

in general. Thus far ferns have contributed more to æsthetic taste, ever and always a delight to the eye, than to serve for extensive practical use. Some species are justly reputed, however, to have fine medicinal qualities. To illustrate: within a few months past it has been discovered and strongly confirmed that the *Aspidium rigidum, var. argutum*, found in the Oakland hills, is a powerfully effective yet harmless anthelmintic. In some parts of the world the young root-buds and tender fronds of certain ferns are cooked for food.

Referring again to the use of ferns as contributing to æsthetic taste, let us direct attention to the entertainment and exquisite pleasure to be derived from a close study of ferns, either in their native haunts, or by carefully preserved specimens in herbariums. Bearing upon this subject I cannot do better than to quote the words of John Robinson, who has some fine practical thoughts in his healthful book, "Ferns in Their Homes and Ours": "There is a large class of persons," he writes, "who are so fortunate (or unfortunate, according as they use or abuse the privilege) as to have nothing to do, or to speak more exactly, have to do only what they choose. This class must have a hobby, or they will *rust* out. Another class are inclosed by hard professional work, which leaves them every day tired, and perhaps cross. These should have some outside hobby, or they will become one-sided and crabbed, and these will *wear* out." Botanical literature abounds with instances whose eminent authors have been derived from these two classes of persons. Mr. Robinson continues: "Without an object we walk aimlessly, we read aimlessly, and we work aimlessly. Without a hobby no great man would *be* great. Every person, old or young, outside of an insane asylum should have some one thing in which an intellectual interest is taken. Forced to study what we detest, as often happens at school, we not only lose the time spent, but a listless habit is engendered; but if taken at the point on which our interest can be excited and led by skillful hands and clear heads, those whose lives would otherwise become dull and trivial, can be indirectly guided to much higher aims and attainments." The "fern mania," as it is called, which may be traced from Europe across the Atlantic, to its recent development in America, is a hobby vastly superior to most others.

The fern hobby, properly guided, can be the means of stimulating pure and healthy exercise, or pleasant and entertaining study; but whether pursued as a pastime or a study, in any event it can do no harm, and may be the cause of great and permanent good.

In Syracuse, N.Y., there is a fern club, composed mostly of ladies, presided over by Mrs. Rust. This club has made extensive collections by discoveries and exchange. The members pursue the study with such zest, pleasure, and success, that the knowledge of their club is spread world-wide, and its example is being extensively emulated.

THE GRASS CROP

METHODS OF SEEDING.

THE practice of sowing clover seed on the snow in March is often practiced, and where the soil is of such a nature that the surface is left broken by frost, and is full of shallow cracks, into which the seed may fall, the practice is an excellent one, otherwise a large proportion of the seed may be wasted. Clover seed lying exposed on a hard surface through a warm rain followed by cold, dry weather, will very surely be destroyed. Clover seeds are closely related to beans, and, like them, are easily destroyed by alternate soaking and drying. Clover seed remaining in the hull as where it is shed naturally in the field, may remain sound for many months. Other grass seeds enclosed in a hull, like redtop, orchard and June grass, may lie exposed on the surface for a long time in cool weather without losing their vitality, and if these are sown in the early spring, will usually germinate. It is, however, generally desirable to roll land where grass seed is sown on the surface, as the roller presses the seed close to the moist earth and facilitates its germination.

The sowing of grass seed late in the fall, just before the ground freezes, is a practice which has frequently been tried with fair results. The chief objection to the practice is, that one can ever know to a certainty just when winter will set in. If sown too early so the seed sprouts, there is great risk of winter killing, the plants being too young and tender to withstand alternate freezing and thawing; while if the operation is postponed, a sudden freeze may close the ground and prevent the sowing being done before the following spring. This method is advocated chiefly by farmers who have land that needs draining, and is therefore too wet to work in the spring. Grass sown so late as to remain dormant in the soil during the winter will be subject to the same difficulties spoken of in the case of spring seeding. In this latitude the middle of October is usually the latest date at which grass seed may be sown with the expectation of obtaining a full crop the following summer, and the succeeding six weeks must then be very favorable or disappointment will follow. On very rich land grass sown as early as July is liable to need cutting the same season, which is always objectionable, as the plants are apt to take on the same habit of growth as when sown in the early spring. The buds are too few, and the joints too long. From the first of August till the middle of September, the farmer has the *very best weeks in the whole year* for sowing grass seed, the earlier date being better for land of medium fertility, and the latter for that which is very rich and mellow.

Sowing with turnips, and between the rows in corn-fields, are methods that have their advocates, and good mowings have been made by each. The main crops, however, must not be over luxuriant or they will so shade the ground as to greatly enfeeble or utterly destroy the young grass. He who adopts the method of growing two crops at once on the same land must not expect full crops of either. Such farming is a sort of compromise, yet sometimes advisable. Since grass and hay have become crops of prime importance, and are no longer deemed as being only worthy of secondary consideration, it would seem that we should treat them as well as we know how. For this reason we do well to sow the seed at the best season, and on the very best prepared soil. Many farmers have failed of obtaining as good crops of grass as they might had they given the ground at seeding time better preparation. Grass seeds are very small, and the plants are exceedingly tender and minute when young, and if they are compelled to struggle for existence among coarse lumps and clods, and in soil that is filled with the seeds of rank growing weeds, full and early hay crops cannot be expected. To secure the best results, the land should not only be rich, but it should be as fine and mellow as the best garden soil. It can not be too deeply worked if it is rich and mellow from top to bottom. But it may be overworked in very dry weather, and become so much like an ash heap that the seed can not germinate. Under such circumstances it is sometimes advisable to wait for a rain to moisten the surface before sowing the seed, though often the farmer who waits is the loser. The light shower will usually do more good just after than immediately before sowing.

The depth to which grass seed should be covered must always be a matter for the exercise of good judgment on the part of the farmer at the time the work is done. On a moist soil in early spring, and with a heavy iron roller to follow, surface seeding is as good as need be, while in the summer or fall, when the earth is light and dry, an inch will never be too deep, and even two inches on some soils would be better. It would be preferable, however, to have the soil in such a condition that the seed would come up freely, covered not more than a half inch. This brings the crown of the plant where nature designed it should be, very near the surface. When the soil is in proper condition as regards moisture, a light bush, a fine-toothed smoothing harrow, or even an iron roller, will cover the seed sufficiently.

HOW MUCH SEED.

The question of thick or thin seeding is one that, like many other agricultural problems, has never yet been settled to stay settled. It is, doubtless, possible to sow too thickly, but like over manuring is rarely done. To us it seems the height of folly to scrimp in the amount of grass seed sown. True, seed costs money, and it should not be wasted, but the cost of seed compared to the value of a full crop, is too insignificant for consideration. If hay is worth twenty dollars per ton, and two tons per acre is a fair yield, requiring in seed an outlay of four dollars, it would certainly be very unwise to sow but two dollars' worth of seed, and consequently cut but one ton of hay.

But the old system of sowing grass seed with grain, and waiting a whole year for the first hay crop, there was time given the grass plants to tiller and spread themselves over the soil, but under the most recent method of sowing grass alone, and cutting a full crop after a few months of growing weather, it becomes relatively of more importance to secure a thick, full sod the first season. If timothy be sown alone we do not consider a bushel of seed any too much for an acre. Of redtop, we would sow at least two bushels. Of orchard grass, two bushels, and a bushel of June grass with it. Rhode Island bent requires less seed by measure than redtop, as the seed is usually much less chaffy. No rule need be given for clover, so much depends upon the amount of seed contained in the land, and in the manure applied.

VARIETIES.

When sowing grass for mowing it is very desirable to select those varieties which will mature nearly at the same period. Orchard grass should not be mixed with timothy and redtop, because there is a full month's difference in their, natural season for ripening, but orchard grass and June grass and red clover may be sown together, and so may timothy and redtop or timothy and Rhode Island.

The selection of varieties is also to be governed somewhat by the character of the soil. Orchard grass, June grass, and clover will each produce, under favorable conditions, two full crops or more in a season, and it would be the height of folly to sow such grasses on dry, hard, poor land, where, from the nature of the soil, a second crop would always be an uncertainty. Put those varieties capable of producing rowen on land that is adapted to continuous growth.

Orchard grass does exceedingly well in certain kinds of soil, and it can be recommended to a limited extent. Its early maturity gives the farmer an opportunity to begin his hay harvest a little in advance of the time for cutting the main crop, which relieves him somewhat from the usual hurry at that season. June grass is especially valuable in pastures, and on moist land it is sometimes desirable in mowings to fill up vacant spaces. It is particularly adapted to accompany orchard grass. There are many other varieties or species of grass that have a certain value, but for the main crop, it is doubtful if there are any that are, on the whole, better adapted to the wants of New England farmers than timothy and redtop. For pastures it will doubtless pay to sow a greater variety of seed, that there may be a succession of feed through the season.

Timothy has been considered a poor grass for pastures, because it is so liable to be pulled up by cattle when growing but we have seen it growing freely in old Vermont pastures, where the soil is rich and not often subject to injury by drought. Rhode Island bent grass, which so much resembles redtop, that many claim them identical, is an excellent pasture grass. For seeding a lawn we have been in doubt whether to recommend Rhode Island bent or June grass, each to be sown alone; either will make an excellent turf, and no other grasses are needed to grow with them if the land is rich, mellow, and sufficiently moist, and the lawn mower is run frequently over the surface.

The mixtures advertised by seedsmen under the name of lawn or English lawn grass, we have little faith in. White clover is, by some, considered a good plant to put on the lawn, but we are not ready without further observation to speak of it in very high terms. Its creeping stems are apt to cover the ground like cranberry vines, and in ordinary mowing fields are much in the way of the rake. Its most appropriate place seems to be in the pasture.

Red clover is another plant over which there is much dispute regarding its value as cattle food. Chemists rate it very high, and some farmers consider it better than ordinary grass for milch cows, but our experience hardly coincides with this view of the case. We would, however, sow it freely with other grasses wherever it can be made to grow without overshadowing the other varieties. If cut at the proper season, and well cured without much exposure, it will never come amiss.—*N. E. Farmer.*

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